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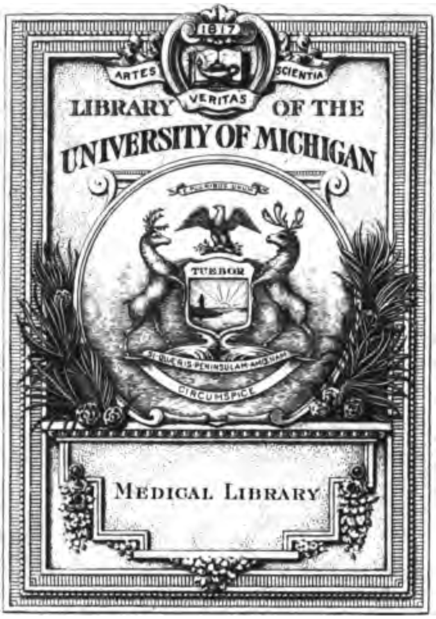

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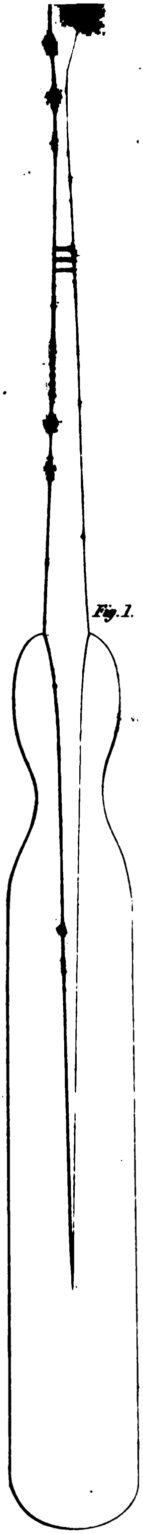
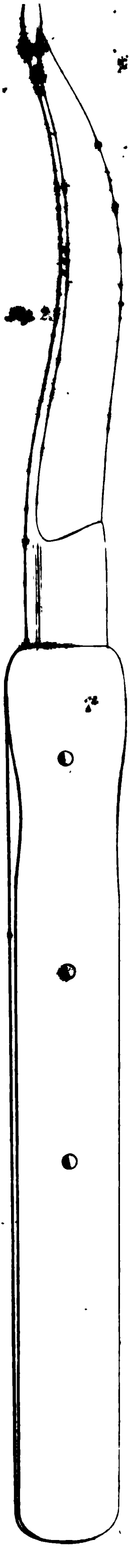


Fig. 1.



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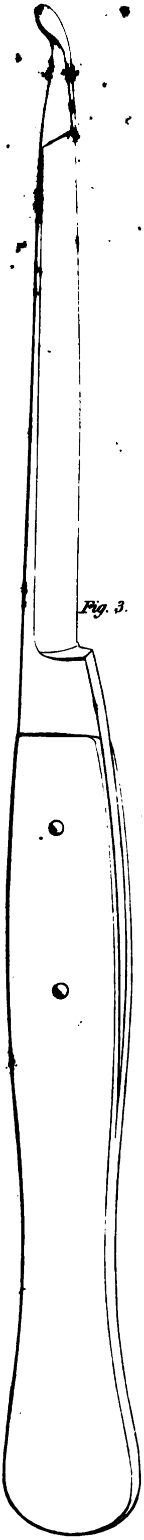

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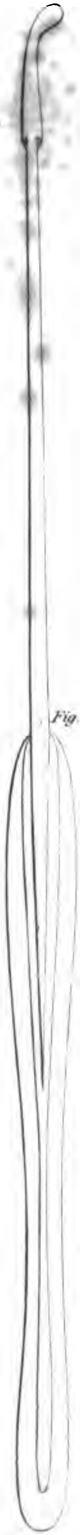

Fig. 4.

Mr. Astley Cooper's
Beak'd Scalpel.

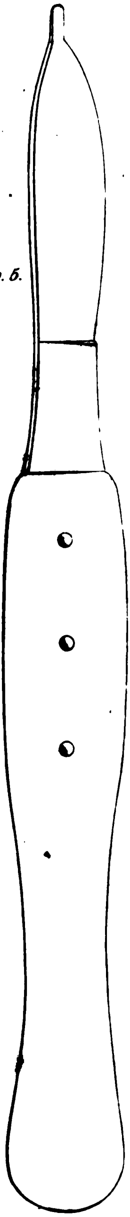

Fig. 5.

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Cooper Sculp.

THE

LONDON
MEDICAL REVIEW.

FROM

JANUARY TO OCTOBER, 1808,

INCLUSIVE.

OMNIA SCIRE, NON OMNIA EXSEQUI; PARVIS PECCATIS VENIAM, MAGNIS SEVERITATEM COMMODARE.
Tacitus, in vita Agric.

VOL. I.

N. Chapman m.

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Dr. R.C. Kent
2-7-1941

PREFACE.

HAVING concluded the first stage of their periodical labours, the Editors of the London Medical Review acknowledge with pleasure the gratification they have received, from the flattering encouragement with which their undertaking has been honoured. The already widely extended circulation of the Work, they feel to be a testimony the strongest and most unequivocal, to the utility of their design and the merit of its execution. By this testimony they are placed under an imperious obligation, to persevere in that line of conduct which they have hitherto invariably pursued: the plan of the Work therefore is by mutual adoption definitively settled. And though the Editors retain undiminished, their aversion to promises and professions, they will pledge themselves to uphold the character of the Work, for every excellence by which it may have been hitherto distinguished.

London, Nov. 1, 1808.

THE

LONDON

MEDICAL REVIEW,

JANUARY, 1808.

N° I.

ART. I. *An Inquiry into the Changes induced on Atmospheric Air, by the Germination of Seeds, the Vegetation of Plants, and the Respiration of Animals.* By Daniel Ellis. 8vo. pp. 246. Edinburgh, Creech. 1807.

CHEMISTRY has thrown, and probably will throw, no direct light on the functions of life. It may teach us to investigate the component parts of the chyle and the blood, and the bile and the urine, but these are only the substances which flow to, and those which flow from, the proper seat of vitality. It may enable us to analyze the circulating fluid, as it goes to the kidney in the form of blood, and comes away in the form of urine, but not to trace it through the labyrinth of secerning vessels, and to observe the mode of its wonderful transformation. We are admitted to the portals of life, but not to the sanctuary where life resides. It is the nervous filament, and the muscular fibre, and the glandular vessel, which are the proper residences of vitality. Sensation, contraction, and secretion, are the three operations of life.

Perhaps in the whole range of medical philosophy, it is impossible to lay our hands upon a more enticing subject, than that which is discussed in the volume we are about to examine. How is the atmosphere changed by living organised beings?

The principal part of those experiments, which tend to throw any light upon the changes which go on in the air, during the processes of germination, vegetation, and respiration, are brought together within the compass of one small volume, a few original experiments are sprinkled in, and the inferences which they appear to lead to, are attempted to be drawn. The narrative of experiments is drawn up with great industry, and the argument is conducted

ducted throughout with clearness, simplicity, and sometimes keenness.

Since the publication of Dr. Priestley's experiments on air, it has been commonly supposed, that whilst the respiration of animals was constantly robbing the air of its oxygen, the vegetation of plants was as constantly restoring it. That animals fed upon oxygen, and emitted carbonic acid; that vegetables fed upon carbonic acid, and emitted oxygen; and that the vegetable and animal kingdoms were constantly engaged in a mutually useful barter.

"Dr. Priestley, whose experiments and discoveries first opened the way to an investigation of the reciprocal changes which take place between living vegetables and the air, was led to conclude, that plants not only lived in nitrogen gas, or what he called phlogisticated air, but that, during their growth, they restored to the air the pure part of which it had been previously deprived by the processes of combustion and respiration, and by the decomposition of organized bodies. He found, that if a sprig of mint was put into a jar of air, inverted over water, and in which a candle had previously burned out, the air was in a few days so far restored that another candle would burn in it perfectly well. He also put a sprig of mint to grow in air wherein mice had died, and in eight or nine days a mouse would again live very well in this air, though he died in another portion of the same air in which no mint had grown. In air strongly tainted by putrefaction, sprigs of mint have sometimes presently died; but if this do not soon happen, they thrive in a surprising manner. These facts led him to conclude, that plants, by their vegetation, reverse the effects produced on the air by the several processes above described, and thus become the chief means by which the purity of the atmosphere is maintained.

"The experiments from which this conclusion was drawn, were made so early as the year 1771, before Dr. Priestley had discovered any correct means of analyzing the air; nor does he seem to have been aware, that the effects produced on the air by the living functions of animals are very different from those which arise from the decomposition of organized bodies. However much, therefore, we may be disposed to admire the beautiful provision which this supposed reciprocity of action would establish between the functions of animal and vegetable life, we must suspend our belief in its truth until better proofs are brought forward than these experiments afford. Even the fluctuation of opinion on this point, which Dr. Priestley himself experienced, may well suggest doubts of the correctness of his views; for towards the end of the same year, some experiments of the same kind, he observes, did not answer so well, and the restored air relapsed to its former noxious state, which led him to suspend his judgment concerning this power in plants. In 1772, on resuming his experiments, he had the most indisputable proof of the restoration of putrid air by vegetation; but hearing that Mr. Scheele and others had obtained different results, he again repeated them in 1778.

The experiments of this year were unfavourable to his former hypothesis; for, whether the air had been injured by respiration, the burning of candles, or other means, it was not rendered better by vegetation, but worse: and the longer the plants continued in the air, the worse it became. He tried a great variety of plants, as sprigs of mint, spinach, lettuce, and some others; but with no better success. His method was to put the roots of the plant into a phial filled with earth and water, and then to introduce it through water into the jar containing the air on which he was making the experiment. In some cases, the air was certainly meliorated, but Mr. Scheele, he adds, always found it injured: and he concludes, that, upon the whole, it is *probable* that the vegetation of healthy plants, growing in natural situations, has a salutary effect on the air in which they grow. Thus we see, from Dr. Priestley's own representation of facts, related with his usual candour, that nothing certain can be inferred from his experiments in favour of this supposed purifying power in plants: and that his conclusion was, even at that period, in direct opposition to the experience of the celebrated Scheele.

"Notwithstanding, however, the uncertain, and, in many respects, contradictory evidence on which this conclusion has been shewn to rest, few opinions in modern science have obtained a more general belief: and both physiologists and chemists seem, in this instance, to have satisfied themselves with contemplating at a distance the beauty of the *final cause*, instead of approaching to a nearer examination of the facts on which the opinion has been maintained. Accordingly, this opinion still keeps its ground, and, in no publication that we have seen, has its accuracy been even questioned. In the experiments, however, which we are about to detail, it will be seen that their results are altogether opposed to the notion that plants, by their vegetation, purify the air; and these experiments have been so often repeated, and with such uniformity of result, as to impress us with an entire confidence in their truth. We proceed, therefore, to investigate the actual changes which atmospheric air undergoes in vegetation; and, first of all, the effects produced on its nitrogenous portion. It has been already shewn, that seeds do not in the smallest degree germinate in nitrogen gas; and Dr. Ingenhousz proved, also, by experiment, that, in pure nitrogen gas, plants do not grow. This conclusion has been more lately confirmed by the experiments of Mr. Gough. He confined several succulent plants in jars of nitrogen gas when their flowers were just ready to expand; but they died away without putting forth any of their blossoms. Others were placed in similar situations before the flower-buds formed upon them, but made not the least effort towards vegetation; and many more, which grew very well in inverted jars of atmospheric air, ceased to vegetate when transferred into jars of nitrogen gas. But this gas, although it does not aid vegetation, appears in no degree to injure the faculty of growth in plants any more than in seeds; for a slip of spearmint, which had remained twelve days in nitrogen gas, recovered upon being restored to the air; and an offset of *sempervivum* vegetated freely after being removed from a jar of the same gas, in which it had been kept from the 2d of April to the

2d of May. As thus it appears, that nitrogen gas exerts no direct influence on plants, so it is probable that they produce no change upon it; and we may conclude, therefore, that this gas, in its simple and uncombined form, is altogether inactive in the process of vegetation.

"We have next to inquire into the necessity and use of oxygen gas, and the changes which it undergoes in vegetation. It has been shewn by Dr. Ingenhousz, that plants do not, any more than seeds, grow in any species of air unless it contain a portion of oxygen gas, and this gas have access to their leaves; that they vegetate very well in atmospheric air and in oxygen gas, and, therefore, that this latter gas is necessary to vegetation. What then are the changes which it suffers in that process? By the experiments of Dr. Woodhouse, we are taught, that plants growing in earth and confined in a glass vessel of atmospheric air render it impure, and that carbonic acid is formed: that when confined in oxygen gas, previously well washed in lime-water, carbonic acid is, in like manner, produced: and that the quicker this acid is generated, the sooner does the plant die. These facts derive confirmation from the following experiment:

"Some mustard seeds, which were raised on moistened flannel, and had grown to more than an inch in height, were introduced into a jar of atmospheric air, and supported about half way up it by a small whalebone hoop, covered with netting. In a deep dish, filled with water, and appointed to receive the inverted jar, was placed a glass of lime-water, which stood directly under the hoop. In four hours, a thickish pellicle overspread the lime-water, and the inside of the jar was bedewed with moisture. At the end of twenty-four hours, the lime-water was very turbid, and the water, into which the jar was inverted, had risen nearly half an inch. These appearances increased during the two succeeding days, at which time the water stood nearly an inch high in the jar. The plants now began to look sickly, gradually drooped, and, by the ninth day, fell down in different directions against the sides of the jar, and, when withdrawn from it, emitted a putrid smell. The residual air extinguished a taper, rendered lime-water turbid, and suffered thereby a farther loss of bulk. This experiment was repeated, by confining mustard plants in the same manner in jars of air inverted over mercury. The lime-water was, in like manner, rendered turbid, and the mercury for two or three days continued to rise into the jars: after a certain time, however, it became stationary, and then began to fall, and the plants when withdrawn yielded a putrid smell."

The fact, then, seems to stand thus: seeds during germination, plants during vegetation, and animals during respiration, produce the same changes on the air in which they live. From the simplest plant which has been subjected to experiment for this purpose, through all the intervening links of organized existence up to man, animated beings are continually changing the atmosphere by abstracting oxygen, and by affording carbonic acid. Whence does this carbonic acid come? Where does this oxygen go? Is the carbonic acid formed independently of the oxygen which disappears?

Or if the carbonic acid be formed by a union of the oxygen which is supplied by the atmosphere, and the carbon which is supplied by the living seed, plant, or animal, does the union take place within the vessels of the living being, or exterior to it?

These are the questions which seem to have particularly engaged the attention of Mr. Ellis; these are what he has attempted to answer, and these, in spite of his attempts, we fear will continue subjects of lasting disputation.

It is the opinion of Mr. Ellis, that the seed, plant, or animal, emits carbon in a minute state of division, which combines with the oxygen of the air, exterior to the organized body, and is thus the cause of that appearance of carbonic acid, and that disappearance of oxygen, which constitute the changes produced on the atmosphere by the growth of seeds, plants, and animals.

The arguments by which Mr. Ellis supports this opinion are too much scattered to allow of quotation, or we should have been happy to allow him to speak for himself. We have endeavoured to collect them together, and shall state them to our readers as clearly, as concisely, and as fairly as we can.

In the chapter on Germination, the chief arguments are these: This process cannot take place without the presence of oxygen, and the quantity of that gas which disappears, equals, as nearly as we can calculate, the quantity which enters into the composition of the carbonic acid which takes its place. The oxygen which disappears, cannot enter into the structure of the seed. It can enter only in two ways; it must be absorbed by the action of vessels, or it must be attracted by chemical affinity. It cannot be absorbed, for there is no structure to absorb, and afterwards to retain it. It cannot be attracted, for there is nothing to attract it but water and carbon, which are insufficient to destroy its combination with the nitrogen. Seeds soaked and immersed in hydrogen, or nitrogen, give out carbonic acid; but this is no living process, for it is not attended by the evolution of the seed. Upon these grounds Mr. Ellis denies the entrance of oxygen into the seed, and infers the formation of the carbonic acid to be exterior to it.

In the chapter on Vegetation, Mr. Ellis argues thus: The atmospherical air cannot enter the plant, for there is no organization which will allow it. The parts of the plant which produce the changes on the air are the leaves, and as far as we know the anatomy of these organs, they possess only two sets of vessels which terminate by open mouths on the surface of the leaf; these are the vessels on the superior surface, whose office it is to emit, and those on the inferior, whose office it is to absorb moisture. It is obvious that neither of these sets of vessels can perform the function of aerial absorption. If we suppose that another set of vessels is provided for this function, the same vessels must be absorbing oxygen and emitting carbonic acid, two currents must be flowing through these vessels in contrary directions, which would constantly impede, or rather, which would instantly destroy each other; this supposition is wholly untenable. Does the sap in its passage through the vessels of the leaf, attract the oxygen by chemical affinity? No, this is impossible; for chemical affinity cannot be exerted, except between substances in actual contact; and this is so far from being the case with the sap and the air, that they are divided by the combined thickness of the vessels in which the sap flows, the green colouring matter, and over all the epidermis. Besides, how is the carbonic acid to be emitted? We cannot refer this to chemical affinity, nor can we suppose it to be emitted by any living action, for we cannot shew the structure which performs it. There is even another difficulty. How is the oxygen to be separated from the nitrogen before it is absorbed by the plant? We cannot suppose the whole undecomposed air to enter the vessels of the leaf; for of what use could be the nitrogen, if it is rejected again, and how could it be rejected in the exact bulk in which it existed before? As the carbonic acid formed contains exactly the quantity of oxygen which disappears, it is fair to infer that the latter is expended in the composition of the former, and this inference is not a little strengthened by the fact, that plants cannot vegetate without oxygen.

When we come to the vermes, fish, amphibiæ, and the hot-blooded animals, the difficulties accumulate upon us. To these all the preceding arguments, and many more are applicable. The mucus which besmears the bodies of worms must be an insurmountable obstacle in the way of absorption; and in the case of fish, the air must be separated from the water before it can be taken up, either by absorption or affinity. Those animals which are torpid during part of the year, produce no change in the air during the period of their torpidity; yet at this time their blood is said to be supercharged with carbon, and ought therefore to attract the oxygen, if it is capable of attracting it at any time.

But supposing the oxygen to be absorbed in some way or other, how can the carbonic acid be expelled? there is nothing in the external air or water which can attract it, and we cannot suppose it to be expelled, as an excretion. If the air passes into the blood, it must pass through the vessels and membranes of the lungs. It cannot be absorbed, because of the fineness of the absorbent vessels, the mucus which covers the pulmonary vesicles, and the elastic nature of air itself. Besides, if air did pass through the membrane of the lungs, it would rather pass into their cellular substance than into their blood vessels, because in the one case it has to pass only through the thickness of the membrane of the lungs, and in the other, both through this and the coats of the vessels. But it has never yet been proved that venous blood contains air, much less carbonic acid air. Venous blood emits this air only in those gases which contain oxygen; these are the nitrous oxyd, atmospherical air, or pure oxygen itself. Dr. D. Darwin's experiment however is a positive proof that venous blood contains no air. "He enclosed a portion of the jugular vein of a sheep between two ligatures; it was cut out, stripped of its adhering cellular membrane, and then thrown into a glass of water of temperature 100°, standing under the receiver of an air pump. It at once sank to the bottom, and did not rise when the air was exhausted; nor when afterwards taken out, wiped dry, and laid on the floor of the receiver, did it exhibit any swelling under the exhaustion of the vessel." Besides, the presence of air in the blood vessels, so far from being salutary, is really fatal to life, Girtanner injected air into the jugular vein of a dog; convulsions were immediately produced, and death shortly followed. The experiments of Bichat, performed in a different way, led to a similar result.

Such is the strain of argument by which Mr. Ellis attempts to prove that no air enters the seed, plant, or animal, during their appropriate living processes, and by which he is led to infer, that, during germination, vegetation, and respiration, carbon is emitted as an excretion in a state of minute division, combines with the oxygen of the atmosphere, and forms carbonic acid. If we understand him right, he supposes that the moisture which is continually evaporating from the surface of seeds, the leaves of plants, and the lungs of animals, carries up with it the carbonaceous excretion which afterwards combines with the oxygen; for he says, that as the carbon of manure is soluble in water, it is probable that the excreted carbon is soluble also, and that the water answers the purpose only of a balloon to the carbon. To the old objection that carbonic acid cannot be formed at the temperature of the atmosphere, he answers, that even if formed during the circulation, it is formed at a much lower temperature than burning charcoal, and its production by seeds, plants, cold-blooded animals, and fermenting substances, is a sufficient reply to this objection.

Having thus heard Mr. Ellis's arguments, we request the attention of our readers (for a few minutes) whilst we state our opinion of their value. Of the two modes in which oxygen is supposed to enter the blood, or the sap, that by chemical attraction seems to us the only probable one; and our author in conducting the argument has, if we are not very much mistaken, rather asserted than proved the impossibility of this mode of entrance. Chemical attraction it is said can be exerted only between substances which are in actual contact, and this is so far from being the case with the air and the sap or blood, that they are separated from each other by the thickness of several membranes. Whether this chemical dogma is true or not we cannot stop to examine, but that the air either wholly or partly attracts or is attracted by some part of the blood, seems to us very clear. Of this we are satisfied by the well-known experiment of Dr. Priestley, who found that a piece of thin membrane interposed between air and the crassamentum of venous blood did not prevent the usual changes. But Dr. P's. piece of membrane was dead; the membrane of the lungs and the coats of the vessel are living. For our parts we should be inclined to have recourse to an argument which we should call an experimentum crucis, but which would be stigmatised by our antagonists, although we are persuaded unjustly, as a petitio principii. Look at the lungs of an animal with the venous blood streaming through their veins, apply hydrogen, or nitrogen, or let the lungs be empty, the blood preserves its purple hue. Apply oxygen mixed or unmixed, the blood instantly becomes florid. Here the interposing membranes are living. How can the oxygen thus act on the blood unless by chemical attraction? If we suppose, with Mr. Ellis, that carbon is emitted by venous blood in a state of minute division, how can the oxygen facilitate this emission, unless by attracting the carbon? Perhaps our author believes that this carbonous excretion is assisted by the stimulus of oxygen, which acting on the vessels of the lungs as jalop acts on the bowels, is in truth a pulmonary purgative. But that blood and air can mutually exert a chemical attraction, either directly or indirectly through an intervening substance, we do not want Dr. Priestley's experiment to prove. Mr. Davie found that the crassamentum of venous blood, although covered by the whey of milk, was still rendered florid by oxygen. Mr. Coleman laid bare the jugular vein of a dog, removed the surrounding cellular membrane, blew upon the vein a stream of oxygen, and notwithstanding the small surface of blood to which the oxygen was applied, found that the colour of the blood was considerably heightened. Those also who have made experiments on animals, and have had occasion to examine the brain immediately after the cessation of motion and apparent sense, whilst every part of the body retained no small portion of the vital principle, have often seen the veins of the pia mater, on being first exposed to the air, completely purple, but a few minutes after assume a visible degree of floridity. All these cases seem to us undeniable proofs of the possibility of chemical attraction going on between the blood and the air in a living animal, because we cannot explain upon any other satisfactory principle, why oxygen more than hydrogen should produce these changes, (in the blood). Whether the oxygen is directly absorbed by the blood, or by the moist membranes, and by them imparted to the blood, we cannot tell. To the lattersup position we have no a priori objections, because it is as easy to suppose that a particle of oxygen is conveyed through a number of successive particles of mucous membrane and blood, as it is to suppose that a particle of caloric is conveyed through a number of successive particles of iron. To all the imaginary difficulties to the entrance of air into the blood, either by absorption or chemical attraction, which consist in the fineness of the vessels, the elasticity of air, and the thickness of the besmearing mucus, we reply by an experiment of Mr. Astley Cooper, which was made for a different purpose.

The *tunica vaginalis* of a dog was opened, and air thrown into the abdomen until it became fully distended. The wound was carefully closed, and within a few days the air was completely absorbed. Here it appears by direct experiment, that the elasticity of air, the fineness of absorbent vessels, the thickness of besmearing mucus, and all such obstacles as these, which are stated so confidently by Baron Haller, are incapable of preventing the absorption of air in a living animal. But on this part of the subject we have said enough.

In that part of the volume where Mr. Ellis refers to the experiments of Girtanner and Bichat, to prove that the presence of air in the vessels of a living animal so far from being necessary to life, is fatal to it, he seems in a most unaccountable manner to have overlooked some circumstances which must destroy all analogy between the absorption of air in the lungs, and the injection of air into the vessels.

There is little doubt that in the experiments of Girtanner and Bichat, the air killed as a mechanical agent which impeded the natural functions of the vessels, but on the contrary when air enters the blood, as it passes through the lungs, it becomes chemically combined with the whole or some part of the blood, and hence loses its aerial form and mechanical agency. But when thrown into the jugular why was it not absorbed? The answer is easy and obvious. In the one case the surface of contact between the air and the blood is very small, in the other the surface of contact is vastly extensive. The superficies of the human lungs exclusive of the Bronchiæ is calculated to be no less than 21907 square inches, ten times greater than that of the whole body, and we need not tell so well informed a man as Mr. Ellis how much chemical action depends on the surface of contact. There is another difference between the two cases. When the blood in its passage through the lungs absorbs oxygen, it at the same moment emits carbonic acid. In the experiment of Girtanner, therefore, even if we suppose that the oxygen is absorbed, we must at the same time suppose that the carbonic aid is emitted; still, therefore, unabsorbed air will be present in the vessels, which will mechanically impede their functions, and thus produce convulsions and death. But supposing that the oxygen is absorbed and the carbonic acid retained, the nitrogen of the injected air will remain in its aerial form, and thus there is an end of the question.

We have little to say to Dr. D. Darwin's experiment, which we have always looked upon as very clumsy and very inconclusive. If air is really contained in the blood, which no arguments we have heard appear to us to disprove, it does not follow that this air must escape as soon as the pressure of the atmosphere is withdrawn. Many reasons might be given why such a change ought not to be expected; two arguments satisfy us, and we dare say they will satisfy our readers. In the first place, we know little or nothing of the precise nature of that combination which is supposed to exist between the blood and the air—it may be stronger or it may be weaker—a priori, the one is as probable as the other, and the reason why a vacuum is unable to separate the air from the blood may be because they adhere to each other too strongly; the removal of atmospherical pressure being capable of overcoming combinations only of a certain strength. In the second place the quantity of air bears but a trifling proportion to the quantity of blood with which it is supposed to be combined. The force with which the air is retained by the blood is, cæteris paribus, as the quantity of the retaining body; and in the case which we are arguing, the comparative quantity of the blood may be so great as to hold the air with a force which the removal of the pressure of the atmosphere may be unable to overcome.

In all the chemical theories of respiration the great difficulty has been to explain the formation of carbonic acid at the common temperature of the atmosphere, and in reply to this objection it has been usual to urge, that, whether it is formed exterior to the vessels, or during the circulation, the union of the carbon with the oxygen must take place at a temperature much below that of burning charcoal; the difficulty applies to one theory as well as to the other, and hence the inference has been drawn that it applies to neither. According to the theory which we are disposed to think the most probable, it is held, that the blood in its passage through the lungs absorbs oxygen, which accompanies it to the most minute ramifications of the arteries; that here the oxygen quits the blood, combines with the carbon which is deposited in the circulation, and forms carbonic acid which is afterwards emitted by the blood on its return to the lungs. Whilst the blood is passing through the minutest branches of the arteries into the veins, is there not present a cause sufficient to explain the union of the oxygen and the carbon, which cause is not present exteriorly to the vessels? It is well known that the vessels of living organized beings, vegetable and animal, possess the power of separating the simple principles of their circulating fluids, combining them anew, and thus of forming substances which did not previously exist in the fluids, from which they are extracted. It is by this faculty, that from the blood are formed bile, and urine, and all the peculiar contents of these secreted fluids. In short the minute vessels of a living animal are laboratories in which are going on multitudes of decompositions, and recompositions, which the chemist is unable to effect. It is probable that by this faculty of the minute branches of arteries, the carbon and the oxygen of the blood are united so as to form carbonic acid.

That these substances are combined by some means or other is certain, but the difficulty is to discover an adequate cause. It is usual to refer their combination to the state of minute division, in which the carbon of the blood is presented to the oxygen of the air; but every one who considers this theory, must feel a doubt of the adequacy of the cause. That the cause to which we have referred this chemical combination is sufficient and more than sufficient to produce the effect, no one can doubt for a moment. By this faculty of transformation, which resides in the vessels of a living being, it is, that the blood of animals is converted into bone, cartilage, tendon, muscle, gland and nerve; that the sap of vegetables is transformed into all the various parts of vegetable structure, into the most delicious fruits and the most fragrant flowers. How then are we to explain the formation of the carbonic acid which is the produce of respiration? That the power of composing resides in the minute branches of arteries is certain; that this power is equal to the effect is as certain. Are we then to refer the composition of carbonic acid to this power, and say that in the same way as uric, so carbonic acid is formed? It is probable and only probable, and all the chemical theories which have ever been formed are *mere probabilities*. All that we know is, that during respiration, carbonic acid is added to the respired air, and oxygen is subtracted; the rest is sheer conjecture.

The only argument which remains to be examined is this: that venous blood emits carbonic acid, only when exposed to oxygen or to some gas which contains oxygen, from which it has been inferred that this blood contains no carbonic acid, and therefore that this acid must be formed exteriorly to the circulation. In the theory which we have been endeavouring to defend against the vehement assault of Mr. Ellis, it is supposed that the carbonic acid, during its journey to the lungs, exists in a certain state of combination with the blood, the precise nature and degree of which combination we are ignorant of. The liberation, therefore, of carbonic acid, and the absorption of oxygen, may be a case of single elective attraction.

The carbonic acid is united to the blood, but this union is dissolved by the presentation of oxygen, which has a greater affinity for the blood than carbonic acid has. Carbonic acid, therefore, will not be emitted by venous blood, unless it be exposed to the action of some substance for which it has a stronger affinity; this is the case with oxygen; but not with nitrogen or hydrogen. Hence the reason why venous blood emits carbonic acid only when exposed to some gas which contains oxygen, without having recourse to the improbable supposition that carbonic acid is formed exteriorly to the vessels, and at the common temperature of the atmosphere.

Our readers will now be able to explain for themselves, without the aid of Mr. Ellis's theory, why, in respiration, the carbonic acid which is found, equals the oxygen which is lost; why torpid animals do not change the air which surrounds them, and a great many other whys, which there is neither time nor necessity for us to answer. It will be remarked, that throughout the whole of this discussion, we have, with few exceptions, confined our views to respiration, and have almost wholly lost sight of germination and vegetation; but the fact is, that in discussing the question with relation to one, we discuss it with relation to all.

The question which has thus occupied our attention, is the principal one which Mr. Ellis has undertaken to answer. The experiments of others which he has collected, his own experiments which he has narrated, all the materials of his volume, drift towards this one point. We have examined his reasonings as fully as was necessary, and as candidly as was possible, and must confess that the evidence which we have already laid before our readers, in support of Mr. Ellis's doctrine, appears to us unsound and unsatisfactory. We come however, now, to evidence of a more doubtful nature.

"M. Huber observed, that seeds did not germinate in air, which had been previously injured by the respiration of bees, although a quantity of oxygen gas was added to it, sufficient to carry on germination, if mixed with pure nitrogen gas; at first, he attributed this failure to the presence of the carbonic acid which remained in the residual air, but when he carefully removed this acid by first washing the residual air in lime-water, he still found, that although fresh portions of oxygen were added, no germination took place, until after a very considerable quantity of that gas had been supplied. He found precisely the same thing to happen when he employed the residual air that had served for successive germinations, since fresh seeds would not grow in such air until it was mixed with a quantity of oxygen much more than sufficient to carry on germination in pure nitrogen gas. Reflecting on these circumstances, he conceived, that as seeds yield carbon to unite with the oxygen gas of the air during their germination, that substance might also combine with its nitrogeneous portion; and when thus saturated with carbon, the nitrogen might not favour germination if mixed only with its usual quantity of oxygen, because the oxygen gas, having a greater affinity for carbon than the nitrogen, would carry it off from the latter to form carbonic acid, and consequently not leave sufficient oxygen to support germination. This supposition was rendered the more probable, because he found, that when oxygen gas was mixed with nitrogen thus saturated with carbon, carbonic acid was formed, although the two gases had, previous to their mixture, been carefully washed in lime-water.

"That the carbon of the seed had really escaped, and combined with the nitrogen gas in the experiments above stated, is farther evinced by what follows. M. Huber prepared an artificial atmosphere in proportions similar to that of common air, but in which hydrogen was substituted for nitrogen gas: and in this atmosphere he caused successive quantities of lettuce-seeds to germinate, till they ceased any longer to grow. He then carefully washed this mixture of gases in lime-water, until it produced not the slightest discolouration; and, when all the carbonic acid was by this means removed, he added to the residue a portion of oxygen, previously deprived of every particle of carbonic acid, and inflamed the mixture in Volta's eudiometer over lime-water. At the moment of inflammation, the lime-water was rendered very turbid, and a great precipitation of carbonate of lime took place. As no carbonic acid existed in the two gases previous to their inflammation, its production must have arisen from that process; and we must therefore admit, that the carbon which had combined with the hydrogen during germination was again separated from it, and had united with the oxygen gas to form the carbonic acid in question. If, after being thus prepared, the two gases, instead of being inflamed, were placed together over lime-water, carbonic acid was, in like manner, formed; but the same result, which in one case was effected in an instant, required, in the other, a much longer time to produce. It is farther remarked, that pure hydrogen gas burns with a lively white flame, but that the gas which has been employed as an atmosphere in germination, yields a tinge more or less blue, although it may have been washed in lime-water."

As seeds, plants, and animals seem to produce the same changes on the air in which they live, it is fair to infer as a probability, from the general uniformity in the operations of nature, that these changes in the air are produced by the same means in all cases. If it was proved therefore that vegetables formed carbonic acid by emitting carbon into the atmosphere, it would go far to prove that animals did so likewise. To these experiments of M. Huber we have little to say. If accurate, what do they establish, and in what difficulties do they not involve us? This emission of carbon into the hydrogen or nitrogen is supposed to take place when all the oxygen is subtracted, when the plant ceases to vegetate, when it is in a state totally different to the natural. To have recourse therefore to Mr. Ellis's mode of reasoning on a different occasion, this cannot be looked upon as a healthy natural living action. Is it consistent with the rigid, cautious, matter of fact, Philosophy of the day, to take the operations of a diseased plant to explain those of a healthy one? But these experiments involve us in a difficulty from which we do not see the exit. If seeds, plants, and animals emit carbon, and emit it as well in hydrogen and nitrogen as in oxygen or atmospherical air, how comes it that they will not live as well in one air as in another? We advise our author to repeat the experiments of M. Huber; our readers to peruse the volume of Mr. Ellis.

ART. II. *An Account of the Ophthalmia which has appeared in England since the return of the British Army from Egypt.* By John Vetch, M. D. Member of the Medical Society of Edinburgh, and Assistant Surgeon to the 54th Foot. 8vo. pp. 142. London, Longman and Co. 1807.

SINCE the return of the British troops from Egypt in 1801, it has been common to complain, that little has been added to our information by the medical officers attached to that army. The expectation from which such complaints originated, seems to us unreasonable. The hospitals of an army, during a short and active campaign in a distant country, afford it is true, an ample field for observation, but one from which little can in general be gathered. The surgeon, liable to be repeatedly separated from his patients, and when stationary to be overpowered by the number and importance of their diseases, exhausted by anxiety and exertion, and distracted by a multitude of objects, is seldom capable of that minute observation and patient comparison, from which alone we can hope to receive any important addition to our knowledge. The bulk of the British forces remained in Egypt only six months. During this time it was harassed by fatiguing marches and laborious duties, it was burthened by a large proportion of wounded, to whom only a canvas covering afforded protection from the heat of a burning sun; and it was ravaged by ophthalmia, dysentery, and the plague. We therefore wonder not that so little has been written with benefit to the profession, but that so much has been collected by such cursory observers.

Unhappily circumstances now occur more favourable to the study of a disease, hitherto deemed almost peculiar to Egypt. Numerous and decided testimonies leave us not at liberty to doubt that its contagious ophthalmia has been imported into England. We may now, therefore, expect that the sagacious and experienced will combine to scrutinize the nature of a pest which, although it seldom destroys life, seldom leaves the capacity of enjoying it.

Of this the work before us professes to give a comprehensive view, viz. "An Account of the Ophthalmia which has appeared in England since the return of the British Army from Egypt." From so full a title, we expected to have been gratified by collected histories of the disorder as it appeared in different parts of the island, and to have received the instructive conclusions of cautious reflection. We were therefore not a little chagrined, on finding that our author's experience was derived from attending to the disease "*as it occurred in the 2d battalion 52d regiment during three months*;" and that he has run to the press with such culpable haste, that "during a subsequent period of three months, his distance from it had prevented him from inserting such additional observations, as the more extensive opportunities of his present situation have afforded." (Pref. p. 8.) From this essay, then, so far from rationally looking for that information which the Egyptian army has failed to supply, we can only expect partial views and premature deductions; for the author's anxiety to display his knowledge has circumscribed it within the narrowest bounds, and deprived him of those advantages which circumstances denied to his brethren abroad.

The chief objects of this treatise are to show that the Ophthalmia imported from Egypt is essentially different from every other form of Ophthalmia; that it is always produced by the contact of a specific purulent matter; that its symptoms are uniform in kind, though varying in degree, and that the successful treatment of it differs widely from that which is usually employed.

These general assertions have by no means been proved. The testimony given by Dr. V. does not even convince us that the disease which he describes was derived from the Ophthalmia of Egypt. The battalion to which he was attached was exposed to many of the common causes of the disease; but to these he does not ascribe it, because other regiments occupying the same barracks were not affected with it during his *three months of experience*;

vience; it must be remarked, however, that one of them had been previously, and was so afterwards. He therefore attributes its introduction to some healthy recruits who had joined the regiment five weeks before it appeared, and who had formerly suffered from an Ophthalmia supposed to have been derived from troops which had served abroad. That we may not be suspected of lessening the force of the author's data and conclusions we shall give the words used by himself.

"It was soon discovered that many of the Irish recruits had volunteered from regiments in which the disease had prevailed, and had themselves been afflicted with it, though in its milder form. The general testimony agrees that these regiments received the Ophthalmia from being garrisoned along with others who had suffered from it in Egypt and the Mediterranean; and upon particular inquiry, I found that the men in whom the disease first shewed itself in the 52d, had previously been afflicted with it in Ireland. It will be seen from the history of the symptoms, that one of the most unfortunate effects of the disease, is the tendency which it leaves to a relapse on the application of any inviting cause. The origin of the complaint in this battalion receives, therefore, the most satisfactory explanation from the combination of the last mentioned circumstances."

That the disease described by Dr. V. was propagated by contagion the facts mentioned by him do not allow us to doubt; but we see no proof that it had a similar origin. From the rapid sketch of its progress given in his first pages, it appears to have attained by slow degrees, during nine months, that extreme violence which alone he witnessed. During the period of increase, the regiment was harassed by marches and countermarches; the sick appear to have been crowded together, and ultimately to have occupied barracks nearly in the centre of an extensive marsh. Several diseases, of which typhus may be given as an example, though decidedly contagious, admit of what has been called spontaneous production. Whether Ophthalmia, under circumstances peculiarly favourable to its increase, may not become contagious, is a question which has not been decided. Whilst a point of so great practical importance remains at issue, we cannot but regret that such statements resulting from inquiries into minute circumstances, instituted ten months after they had occurred, should be regarded by any medical author as "*most satisfactory.*"

The opinion that actual contact of morbid matter is required to produce the disease in question, in one which, however probable, we think should be inculcated with reserve, both as it tends to diminish precaution, and as it is decidedly opposed by one who has paid early and great attention to the subject. The observations and reasonings of Dr. V. on this question have failed to give us that "full satisfaction," which they seem to have afforded himself.

self. They chiefly rest upon the narrow limits within which the disease was circumscribed. But the limited powers of some specific contagions, as well as those of typhus, have of late been fully established. Many of our readers must have witnessed, as we have, the possibility of inclosing even hospital-gangrene within a single ward, yet we do not thence conclude that sanies must be carried on spunges from one sore to another in order to effect its communication.

Dr. V. divides this disease into two stages, each of which is marked by symptoms widely different from those of common ophthalmia.

"Whether the disease goes through both stages, or is confined to the first, as is frequently the case, the first appearance of the disease is so uniformly the same, that it may appear unaccountable that any difference of opinion should exist concerning the seat of its commencement." P. 29.

The characteristic symptom in the first stage is inflammation, beginning in the insides of the eyelids, and always affording a purulent secretion. Other diagnostic marks are enumerated by the author which appear trivially minute; thus he informs us that "the inflamed vessels assume a colour which approaches more to that of a *light brick*, than is the case in common ophthalmia;" and again, "the same increase in the size, and in the tortuous course of individual vessels, as is general in the commencement of the latter affection, is never remarkable in this." P. 30. During the prevalence of this inflammation, even when it appears alarmingly great, the pupil is rarely changed in its diameter, or the eye pained by light. When the inflammation spreads and increases, it is attended by a sensation as of sand in the eye. "This sensation, though common to every inflammation of that organ, is from its temporary duration, and regular recurrence, peculiarly characteristic of the present affection." "Its departure is in general as sudden as its accession, after which the eye is free from all uneasiness, until the next recurrence of the same symptom." P. 34. The author adds, "In no instance during this stage of the disease, did I ever find any mark which could in the least indicate an affection of the system."

In the second stage the eyelids swell suddenly and greatly; the purulent matter is secreted in greater quantity and becomes thinner; little inconvenience is produced by exposure to light; tears are secreted more copiously; great chemosis occurs; the sensation as of an extraneous body diminishes; and a deep seated violent pain ensues. This pain is regarded by the author as a chief characteristic of the second stage, his account of it, however, is much too prolix to be extracted. It is periodical in its attack;

is sudden in its accession and cessation; is not increased by exposure of the eye to light; and frequently attacks other parts of the head and face, occasionally causing them to swell, the pain being increased on pressure. The pulse is not affected except where bleeding has been employed; nor are the general functions, if we except a very few cases, deranged.

Such is a brief sketch of the leading features in the disease described in this essay. However accurate we may suppose the narrator to have been in the transcript of what he saw; yet we must decidedly object to the foregoing history, considered as a general account of the Egyptian Ophthalmia, either as it appears in this or its parent country. Descriptions of ophthalmia in England traced more distinctly than by Dr. V. to that of Egypt, differ widely from the above, both as to its mode of propagation, its symptoms, and its progress. In Egypt the disease was by no means uniform; on the contrary, it was infinitely varied. Speaking from our personal experience, we affirm that it was generally attended with extreme pain on exposure to light, with frequency of pulse, and general fever. The swelling of the eyelids, and discharge of matter resembling pus, though common, were by no means universal. Sudden, severe, and temporary aggravations of pain occurred in that situation, as they occur often in common ophthalmia, but these were never observed to be regularly periodic with intervals of perfect ease; nor did there ever appear in a single instance, those violent affections of the temples, forehead, or face, attended with swelling, and pain increased on presure, described as common in the work before us.

In the treatment of the disease, every common remedy, and many which are uncommon, appear to have been used in vain. Slight bleedings, general and local; cold and warm applications, opiates internally and externally; bark; purges; emetics, &c. &c. seemed to have failed in giving any serious check to the disorder. A mode of treatment was at length suggested by Mr. Knight, which was attended with singular success. The account of this practice we shall give in the words of the author, premising that those labouring under ophthalmia were divided into classes, and kept in distinct wards; and that great care was bestowed in detecting the first signs of the disease.

"Notwithstanding the undisturbed state of the system, and apparently in opposition to the experience which had been already gained, Mr. Knight proposed the most decided antiphlogistic regimen, with the regular exhibition of purgatives, and above all the use of the lancet with a freedom far beyond what had been formerly thought of." P. 97. "In every case, where on the first appearance of the disease, bleeding is employed to the proper extent, its effects are no less perceptible to the patient than to the surgeon. The diminished vascularity is the first effect which ensues, and before the end of the operation the eye will often become nearly of its natural appearance. The cessation of all uneasiness should be the sine qua non of stopping the flow of blood. This, in a robust man, will often not be obtained until thirty or forty ounces have been taken away; and in a few delirium will take place before it is effected; one or other of these effects should always be procured. By the repetition of this remedy on every aggravation of the disease, whether in the appearance of the eye, or in the sensations of the patient, we may in ninety cases in the hundred, prevent it from arriving at the second stage. If, however, in defiance of this treatment, it assumes its violent form, the same means ought, if possible, to be more fully persevered in. In this stage the patient will often have bleeding to a greater extent; in many, fifty or sixty ounces must be taken away to relieve the pain or bring on syncope; but we can always rely with certainty on the benefit which will ensue when either of these effects are produced. In every case where such practice is employed, however violent the tendency of the disease may be, its fatal termination will infallibly be prevented, and with much less expense to the patient, than by smaller and more frequent bleeding. According to the rules above laid down, for the extent to which this evacuation is to be carried, the quantity taken away must be in proportion to the strength of the patient; hence few cases will occur where this practice cannot be put in execution." P. 100.

On the whole, we regard this treatise as one of considerable value, inasmuch as it contains a full, though scattered account of a disease, which by degrees, and under peculiar circumstances, acquired singular virulence and obstinacy; as well as of the treatment to which it ultimately yielded. By this a real addition has been made to our information. But the author has lessened the value of the history which he relates, as much as was in his power, by converting particular facts into general assertions. That the disease which he describes was not derived from the Egyptian ophthalmia, we do not assert, but we maintain that it differs greatly from the common forms of that affection. Of this perhaps Dr. V. might soon have been convinced, had he been less anxious to print. He says, indeed, in a hurried postscript, that his more extensive experience has confirmed the correctness of the greater number of his observations; and, in consequence, he has not made a single alteration in his general statements. Yet he informs us, in the same hasty manner, that though his patients were more enfeebled by climate, than those in the 52d, intolerance of light and constitutional symptoms were general, and that intermissions of pain were rare. These differences, from his first account, are by no means slight. Ere this time he has probably seen others, and possibly sufficient to make him regret, with us, that he did not publish *a brief* *narrative of an ophthalmia which occurred in the 2d battalion of the 52d Regiment,* divested of futile speculations and sweeping conclusions, rather than "*an account of the ophthalmia which has appeared in England since the return of the British army from Egypt.*"

The deficiencies in our knowledge respecting Egyptian ophthalmia, which existed previously to this publication, appear to us to exist still in nearly an equal degree. The mode of its origin, the immediate means by which it is propagated, the periods at which it begins, and at which it ceases to be contagious; its characteristic symptoms, and more frequent varieties; with the means best adapted to the relief of its different forms, are still undecided. We are willing however to hope, that the treatment of Mr. Knight, which certainly was never employed in Egypt, and especially his careful separation of the sick, may be found more generally applicable to the disease, than any of the dogmas of Dr. Vetch.

A marked contrast seems to exist between the simple views of common inflammation in the eyes, given by nosological and systematic writers, and the infinite variety of forms under which it occurs in practice. By this contrast, and by the opposite remedies required for its treatment, we are induced to suspect that the subject has never received, at least in this country, that close and scientific scrutiny which its importance merits, and which we trust that our popular expedition to Egypt may yet tend to excite.

Art. III. *The Anatomy and Surgical Treatment of Crural and Umbilical Hernia.* By Astley Cooper, F. R. S. &c. &c. Atlas folio, 1807. pp. 90. London, Longman & Co.

It is impossible that a disease of so frequent occurrence as hernia in its common forms, should have excited observation for a period of more than three thousand years, without long since obtaining a well marked character in surgical pathology. Accordingly, in comparing the records of its history and symptoms at very remote periods of time, we discover as little essential difference in the details, as we may suppose to have existed in the diseases to which they refer. We look in vain for the same characteristic uniformity of rationale and treatment. For these more than observation is required. Theory must be influenced by the operation of all those causes which directly or incidentally conspire to the growth of intellect, and tend to elevate each succeeding century upon the shoulders of its predecessor: and the history of Therapeutics presents a variety strongly marking the bias of existing circumstances, and the prevalence of popular opinions over the reasoning powers of mankind.

The most notable stadium of our advancement in the knowledge of hernia, till within the last few years, is in the line of morbid anatomy. Verheyen, who wrote in 1710, first demonstrated the distinct formation of crural hernia, which was before generally confounded with bubonocele. The most eminent authorities of the same era were divided on the questions, whether the umbilical hernia was ever contained in a sacculus? whether the investing membrane of the abdomen was in the other species ruptured or simply dilated? Whether the sac was formed of this membrane, or of condensed cellular tissue? And the appearance frequently observed of the testis and abdominal viscera in contact, although correctly explained in their anatomical relation by the celebrated Mery, (Vid. Acad. des Sciences, 1701,) were enveloped in total mystery as to their cause, until the observation of Dr. Hunter, and the industry of his brother, combined to illustrate the congenital hernia.

We might cite more and not less striking examples of the comparatively recent period within which such points have been ascertained. The ignorance and confusion which have pervaded and obscured all the concomitant descriptions of the sound anatomy, are on the other hand too notorious to elude observation.

The present work comes recommended in the most forcible manner to our attention. We are apt to indulge expectation too freely not to hazard its disappointment, when a practitioner of acknowledged reputation and abundant opportunity, enhances the value of a great undertaking by a proportionate energy and assiduity of execution. Mr. Cooper's local advantages are sufficiently known. It remains for us to shew how well he has improved them. Our readers, we presume, are already acquainted with the former part of the work, comprehending the anatomy and surgical treatment of inguinal and congenital hernia. The interval which has elapsed since its publication has given birth to so free a discussion of the subject, that we deem it superfluous to revert particularly to its contents. Parts however and doctrines, important in their connexion with one, relate more or less to the other species, and to these we may have occasion to allude.

The femoral or crural hernia involves, perhaps without exception, the most complicated structure in the anatomy of surgery. To the observation of Gimbernat, surgeon of the court of Spain, we are indebted for the first scientific illustration of its more important parts, and his example has proved little less beneficial than his knowledge by communicating to surgeons in general a spirit of active and persevering inquiry. Gimbernat described the posterior insertion of the external oblique muscle into the crista of the pubes, and in its sharp, firm, crescented edge, fixed a seat of stricture which has hitherto remained uncontested. The writer under review indirectly conveys this opinion, as we shall have hereafter occasion to shew. A fascia which covers the iliacus internus muscle is connected to the crural arch as far as the artery, and here dips to give passage to the vessels, as was likewise demonstrated by Gimbernat. He further suggested that the prolongation of this fascia constituted the posterior layer of the sheath, which encases the vessels. That the fascia lata does not form as he supposed the proper anterior layer of the sheath, has been long known, but it remained for Mr. Cooper to demonstrate that this layer was no other than the fascia which is fastened to the arch beneath, lines the crescentic tendon within, and spreads upward under the transversalis muscle. This is the fascia transversalis. Posterity will add—of *Cooper*. In the original description its production below the arch is thus briefly stated. "A part of the same fascia descends under Poupart's ligament, and becomes united to the femoral artery and vein." Vol. I. p. 6.

It is difficult to explain the cause of a variety of report, or fluctuation of opinion upon a subject which admits the clearest form of demonstration; and any addition at this period to our knowledge furnishes a strong practical proof of the intricacy that attends the investigation. As our author's account of the fascia lata differs in no very material respect from that previously received, we shall describe it, for the purpose of stating some points of difference which have been hitherto overlooked, and are the result of our personal observation. The very dense and coarse aponeurosis that rises from the outer labium of the spine of the ilium and confines the abductor muscles is connected on the ilial side of the thigh (viewed anteriorly) to the rounded edge of the crural arch. It covers incompletely the sheath of the vessels, but has a process stretching over its mouth, and terminating in a blunt point, at the insertion of the Fallopian ligament into the tuberosity of the os pubis. It presents a natural curvature whose concavity faces the pubes, and under it lie the absorbent glands that are connected with the lymphatics of the groin. The saphæna major vein is just included by its lower edge, and its semilunar margin is not clearly defined until the removal of the glands above mentioned. This process was represented in a plate of Mr. Cooper's former volume, and first described by Mr. Burns of Glasgow. From beneath the saphæna, the facia continued over the triceps muscle, invests the inner and posterior part of the thigh, and finds insertion above in the simphysis, spine, and crista of the pubes. The plane of the pubal portion covering the pectineus muscle, is so much inferior to the ilial, as to form with it a considerable angle. It does not, as some authors have painted, terminate here by a bony insertion, but dipping behind the sheath of the vessels, lines, by a strong aponeurotic web, the hollow space situated between the pectineus and joint tendon of the psoas and iliacus muscles. After forming here the proper bed of the sheath, it mounts upon the iliacus muscle, and becomes incorporated at its outer edge with the ilial fascia lata: ascending upon the belly of the muscle, it is fixed on one side into the inner labium of the crista ilii, and on the other into the brim of the pelvis. Thus the fasciæ lata and iliaca, as they are called, are essentially one and the same continuous production, and appropriating to a single texture a single name, we propose to delineate the ilial, pubal, and iliac portions of the fascia lata. This fascia, it will appear then, from the description, is commenced at the outer, and terminated at the inner edge of the os ilium, and very curiously reflected around the sheath of the vessels. The ilial and a part of the pubal portion protect it on the fore part, the remainder of the pubal and the iliac behind. Vid. fig. 1 and 4.

Describing the iliac fascia, Mr. Cooper says, "Another portion of this fascia descends behind the crural artery, upon the upper and middle part of the thigh, which sends a process forward to pass between the artery and vein." P. 3. Speaking of the fascia transversalis, he adds, "This fascia sends a process before the iliac artery and vein, and another between them." Again, "Each of these fasciæ sends down a portion of its substance under the crural arch, to form the crural sheath for the artery and vein. The fascia iliaca sends the posterior part, the fascia transversalis, the anterior, and by the union of these on each side, the lateral portions of the sheath are produced." P. 4. It is not without a feeling of the utmost deference, that we venture to call in question the accuracy of an anatomist so justly celebrated as the author of the work before us. Our public duty, however, demands a declaration of what we conceive to be the truth as it is found in nature, and such a declaration we are confident no one more earnestly desires than Mr. Cooper. "*Amicus Socrates, amicus Plato, sed magis amica* VERITAS."

We have already endeavoured to convey the idea of continuity of surface between the pubal and iliac portions of the fascia lata. We have now to observe that the sheath is behind perfect of itself, and has no other connexion with the iliac portion than that of cellular membrane, which, below its insertion into the linea ileo-pectinea of the pubes, we have found loose enough to yield to the handle of the knife. Even to the point of its insertion into this line, it is easily separable from the iliac portion of the fascia lata terminating beneath it in the same line, though at this part less cellular substance intervenes.

When the sheath and its vessels are divided at the entrance of the saphæna vein, and reflected upon the abdomen, the fibrous interlacement of the pubal and iliac fasciæ, presents a smooth and uniform surface. Vid. fig. 4. The idea of a piecemeal structure, we confess, never appeared to us sufficiently *natural* to be quite correct.

Nature is always simple. Every vessel has its peculiar cellular investment, derived probably from the aorta and cava*. We doubt if artery and vein are ever really contiguous. Where they are described to have a single sheath, and this sheath to have a septum, what is more probable than that the septum is a condensation of the inner and opposite sides of the peculiar sheaths.

Nature, if we except her lusus, is never redundant. Why do we look for an exception in the formation of the crural sheath, to that of sheaths in general. An evident necessity may be seen for strength on its upper and fore part, but at no other. This it receives in an eminent degree from the expansion of the fascia transversalis†, although possibly even this fascia contributes nothing to its actual formation. The peculiar shape of the crural sheath may be accounted for. The mouth of it on the ilial side is obtusely, on the pubal, acutely angular. For on the former it lies contiguous to the artery at the outer margin of the crural space, whereas on the latter it is extended beyond the vein to its inner margin, so as to include the foramen of the absorbent vessels. Vid. fig. 3. The ring, as it is called, by which the crural hernia descends, is said to have been described as early as the year 1734, by M. Bassuel, in the Mercure de France‡. Its boundaries have been fixed by modern writers, with much precision, but it has often no such definite circular outline as its name would lead us to imagine. Our experience leads to the conclusions, that between the vein and edge of the crescentic tendon, a space is left large enough in the adult to pass the point of the fore finger, and that a lymphatic

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* Winslow describes the cellular investment of the abdominal and crural trunks, as processes of the external lamina of the peritoneum. Vid. vol. ii. p. 163, oct. ed. by Douglas.

† Vid. fig. 2.

‡ Vid. an Essay on Crur. Hern. by William Wood, Edinb. 1804.

Plate I. [redacted] 28.

Fig. 1.

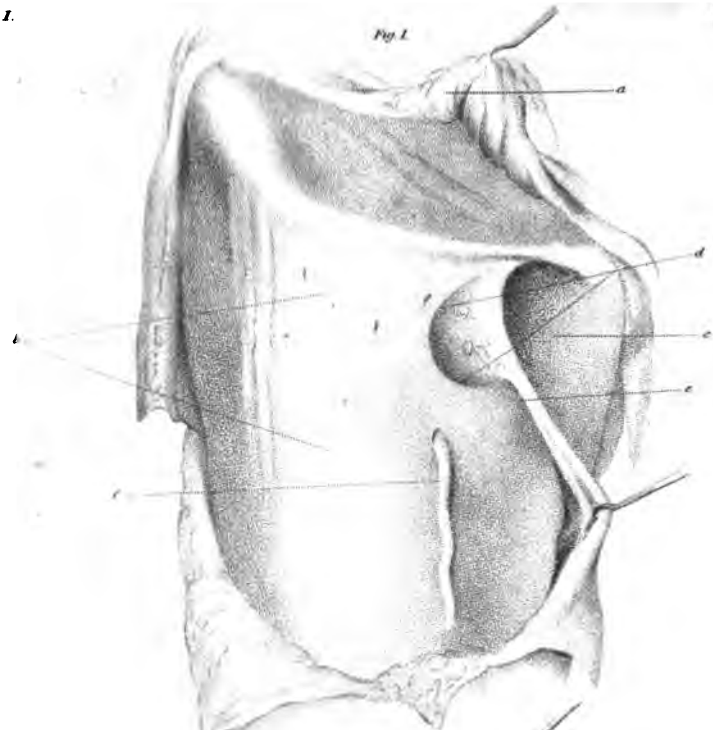
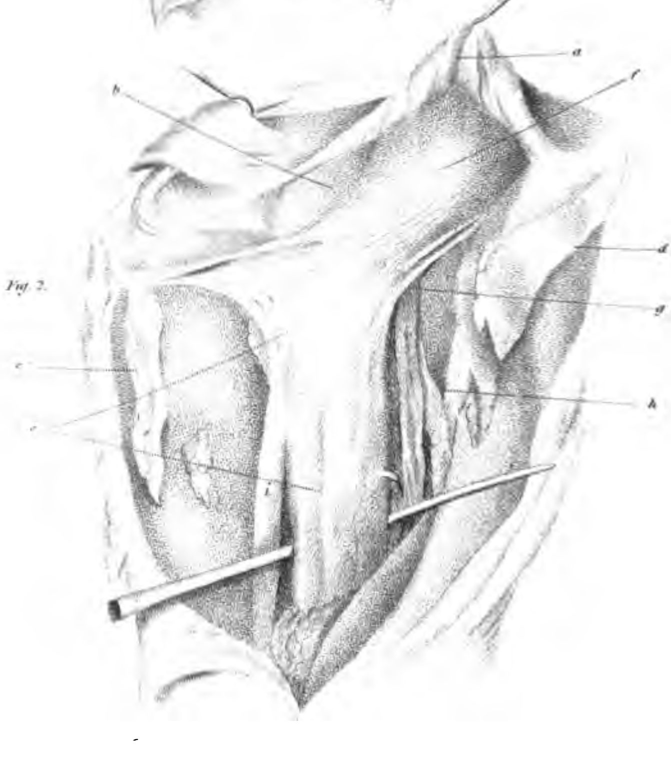


Fig. 2.



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phatic gland contiguous to the vein, occupies a part of it; that a cellular expansion, covering the iliac and psoas muscles and their common fascia, and descending over the front of the bladder, shuts it on the interior, and is of course at the same time protruded by the finger. Upon opening the sheath, a delicate membrane is seen to stretch across this space, and part it from the vein, and it is this third compartment of the common sheath, for the passage of the lymphatics, that explains its internal angular production, and gives it the resemblance to a funnel.

The annexed sketches are designed to illustrate our description. Fig. 1 and 2 resemble, upon a reduced scale, those of Mr. Cooper, and are introduced only for the sake of completing the series. The first sketch is from the right groin of an adult female, and represents the falciform process of the ilial portion terminating in the pubal portion of the fascia lata, under the saphæna vein.

a	Integument raised.
b	Ilial portion.
c	Pubal portion of fascia lata.
d	Falciform edge of the ilial portion.
e	Continuity of these portions under the saphæna vein.
f	A vein penetrating the fascia.

FIGURE II. From the left groin of the same subject, exhibits the transversalis fascia, descending from beneath the muscle of the same name, to traverse and strengthen the fore part of the sheath. Poupart's ligament is here cut across.

a	Transversalis muscle raised.
b	Internal aperture of the abdominal canal.
c	Pubal half of Poupart's ligament.
d	Ilial ditto.
ee	Sheath of the vessels unopened, raised by a blow-pipe.
f	Fascia transversalis.
g	Circumflexa ilii artery and vein, entering the sheath.
h	Anterior crural nerve.
i	Saphæna vein.

FIGURE III. From the right groin of a male adult, presents a view of the sheath laid open, its septum drawn up, and the crural foramen blocked by a gland.

aa	The septum drawn up by a hook.
b	Internal angular insertion of sheath into the pubes.
c	Ilial side of sheath.
d	Crural artery.

e Crural vein.

f Epigastric vein entering the crural vein.

g Obturator vein entering below it.

h A curved probe passed through the angle formed by the epigastric and crural trunks.

i A thin membrane connecting the epigastric and obturator veins.

k Crural foramen.

l A lymphatic gland.

FIGURE IV. Shews the intertexture of the reflected pubal and iliac portions, the sheath and its vessels having been cut across, and turned upon the abdomen.

a Sheath and its vessels turned up.

b The fascia covering the iliacus internus muscle, or iliac portion of the fascia lata.

c Pubal portion continuous with it and forming

d Bed of the sheath.

So much for the parts concerned in the healthy anatomy and their respective illustrators. We next proceed to inquire by what route the crural hernia descends, and by what coverings it is invested. The answer given by our author to the first question is, that the sac, after descending into the crural sheath, turns inwards, and penetrates the inner part of the sheath, where the absorbent vessels pass. Another opinion is, that it passes close to the pubes, exterior to the sheath, and has no connexion with it.

The course of the sac, if it becomes a question, is one which anatomy only can resolve. To the latter opinion, therefore, we reply, by again stating that the orifice, improperly called the crural ring, is included between the vein and the inner boundary of the sheath, through which the absorbents ascend to the loins, and the hernia descends upon the thigh. Fig. 3. That no other natural aperture exists under the inguinal ligament, and that force is even incapable of procuring any other passage for the finger passed from within the abdomen. It is a common, but a paltry pretext for the negligence of anatomical research, to urge that these points are of no material import in practice. The coverings of the sac are described as follows.

"The crural hernia, when dissected, presents the following appearances: when the skin is removed, the superficial fascia of the external oblique muscle is laid bare, which, though it is of a delicate texture in its common state, when pressed upon by a hernia becomes extremely thickened and very distinct, more especially in a subject loaded with fat. Under this covering there is generally another fascia, precisely of the

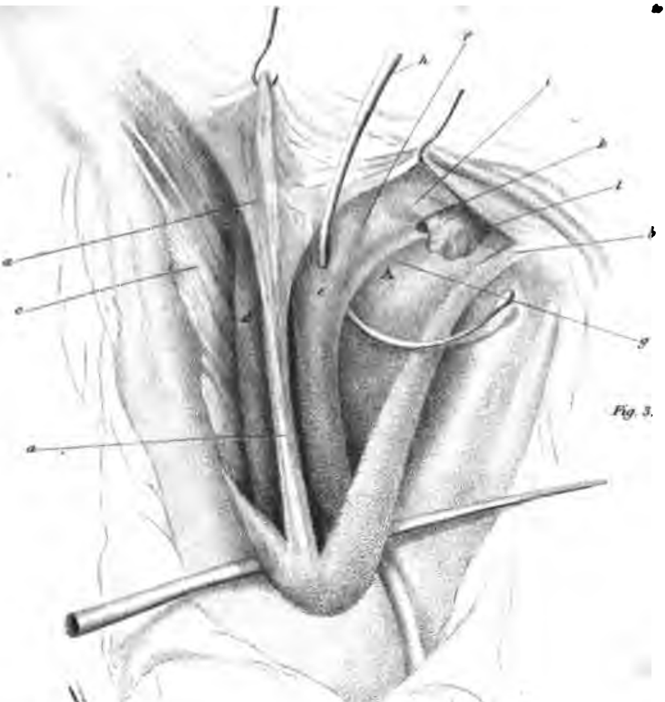

Fig. 3.

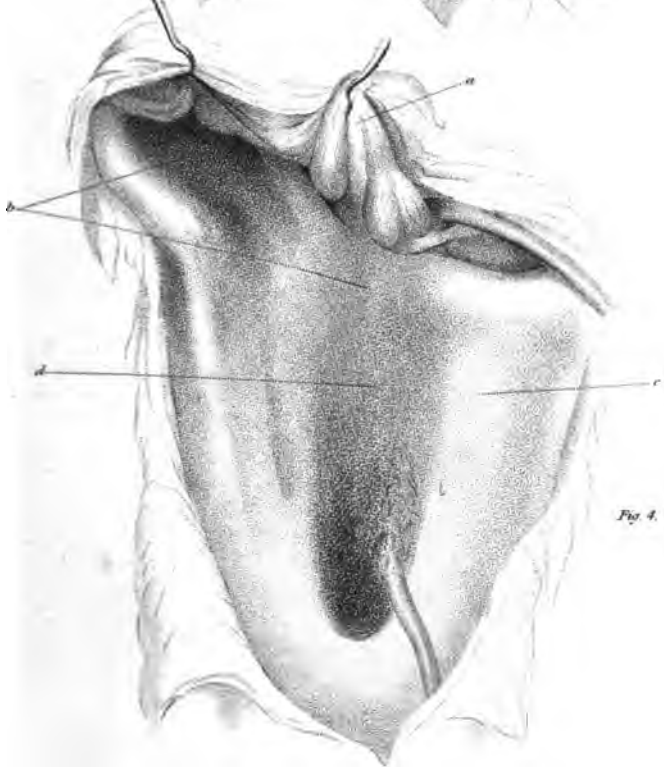

Fig. 4.

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the form of the hernia itself, and which it very closely embraces. A thin fascia naturally covers the opening through which the hernia passes, and descends on the posterior part of the pubis. When the hernia therefore enters the sheath it pushes this fascia before it, so that the sac may be perfectly drawn from its inner side, and the fascia which covers it left distinct. The fascia which forms the crural sheath, and in which are placed the hole or holes for the absorbent vessels, is also protruded forwards, and is united with the other, so that the two become thus consolidated into one. If a large hernia is examined, this fascia is only found to proceed upwards as far as the edge of the orifice on the inner side of the crural sheath by which the hernia descends, but in a small hernia it passes into the abdomen as far as the peritoneum and forms a pouch, from which the hernial sac may be withdrawn, leaving this, forming a complete bag over the hernia. In a small hernia the fascia is thicker than the sac itself, but by being gradually distended it becomes thinner and less distinct, and in one example of this kind from the female subject (which forms the subject of one of my plates), this and the superficial fascia have coalesced into one. I first observed this fascia in dissecting a male subject brought into St. Thomas's Hospital in the year 1800, who had a strangulated crural hernia on the one side, and a reducible one on the other; I next saw it in the operation performed upon Mrs. Bispham, hereafter to be mentioned, and have ever since demonstrated it in preparations whilst delivering my lectures on crural hernia. This fascia will be seen in almost all the plates, and in one of the drawings of Plate IV. very distinctly, as the hernial sac is removed from its inner side. It may be termed the *fascia propria* of the crural hernia. When this fascia is divided, a quantity of adipose membrane is found between it and the sac, and when this is cut through, the peritoneal sac itself is exposed. Behind the hernial sac is the fascia lata, and the sac rests in the hollow between that part of it which covers the crural vessels, and that which passes over the pectineus and triceps muscles, so that the fascia lata is situated posteriorly to the hernia.

This, it must be acknowledged, is by much the most material of our author's discoveries. Of the production of this tunic a twofold explanation has likewise been offered, viz. that it may be formed of the protruded portion of the sheath, or of the membrane that naturally covers the crural opening. The former hypothesis it will be seen applies exclusively to our author's explanation of the course of the sac, the latter to either. It may be said that the existence of the proper tunic adds nothing to the probability of the sac passing through the sheath, since the cellular membrane of the ring, though in its natural state a texture of great tenuity, becomes thickened by the inflammation of moderate pressure. We are not reduced to the necessity of urging this supposition, since it follows from the statement already made, that the sac must first pass into the sheath of the vessels, and consequently, if it appear at all, that it must protrude it*. Here, then, is the simple explanation of the fascia propria. The very delicate membrane of the ring may be set wholly out of view: if it did not exist or existed only partially, having one large foramen instead of many foraminula, or if it were even ruptured by the descent, still the origin of the fascia propria is certain, and a crural hernia can never exist without it. Having cleared the point of its formation, the following observations we think forcibly enough exemplify the necessity of knowing its structure.

"When this fascia is divided, the tumour is so far exposed that the circumscribed form of the hernia may be distinctly seen, and a person not well acquainted with the anatomy of the parts would readily suppose that the sac itself was now laid bare. This, however, is not the case, for it is still enveloped by a membrane, which is the fascia that the hernial sac pushes before it as it passes through the inner side of the crural sheath. This membrane, the fascia propria, is to be next divided longitudinally from the neck to the fundus of the sac; and if the subject is fat, an adipose membrane lies between it and the sac, from which it may be distinguished by seeing the cellular membrane passing from its inner side to the surface of the sac. This is, in my opinion, the most difficult part of the operation, for the fascia propria is very liable to be mistaken for the sac itself; so that when it is divided, it is supposed that the sac is exposed and the intestine is laid bare; following up this idea, the stricture is divided in the outer part of the sac, and the intestine, still strangulated, is pushed with the unopened sac into the cavity of the abdomen. I have given a plate, from an excellent preparation now in my possession, of an undivided sac returned into the cavity of the abdomen, and containing the strangulated intestine, and the particulars of the disease at the end of this part of the subject.

"It must be remembered, however, that in large herniæ the fascia propria of the sac is sometimes inseparably united to the superficial fascia, so that the same incision divides both."

We shall not detain our readers by retracing the distinctive characters of this disease, but may observe on the diagnosis, that we perfectly coincide with our author in esteeming that error to be the most dangerous, which he finds to be unfortunately the most frequent.

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* We have met with herniæ in the subject that had not penetrated the wall of the sheath. The possibility of overlooking femoral hernia under these circumstances may be easily conceived. Here the sheath undilated forms the fascia propria. This stage of the descent is analogous to that of the inguinal, when occupying the interval of the upper and lower apertures of the abdominal canal.

"But the error which is the most frequent, and by far the most dangerous, is that of mistaking crural for inguinal hernia. It is dangerous during the operation of the taxis, as the direction of the pressure should be entirely different in the two diseases; and it is extremely dangerous in the operation, particularly if the surgeon's usual habit of operating in inguinal hernia is to make the incision upwards and outwards towards the spinous process of the ilium. Several instances of such mistakes have come to my knowledge. I once went with a physician into the country to operate, as I was told, on a case of inguinal hernia, but when I examined the patient, I found that the hernia was crural, and had been three days strangulated, during which time repeated attempts had been made to reduce it by pressing the tumour towards the spinous process of the ilium. It was reduced in five minutes by employing the pressure proper for the crural hernia.

"A Surgeon who had operated on a crural hernia sent his patient afterwards to London, to request that a proper truss might be procured for her, and in his letter stated, 'that, during the operation, he had found the stricture in this case at the abdominal ring.' He was fortunate in having escaped destroying his patient. Another Surgeon in operating in a supposed *inguinal hernia* had a venous hæmorrhage arise, which delayed the operation fifteen minutes, and which he found it was difficult to stop. It was a crural hernia, and it was probably the crural vein which he had injured, as he cut towards the spinous process of the ilium. This dangerous mistake is liable to happen without considerable attention, particularly as the crural hernia, after descending into the thigh, often turns before the crural arch. The marks of distinction, as far as I have been able to observe, are the two following: first, that the neck of the inguinal hernia is situated above the tuberosity of the pubis, but that of the crural below it and to its outer side; and secondly, if the sac is drawn down in the crural hernia, the crural arch may be traced above it. If these two circumstances are carefully attended to, the disease will not be confounded by those who have studied the natural structure of these parts."

The dimensions of the parts, both hard and soft, are found to be greater in the female than in the male inguen. Partly owing to this cause and in part to the different economy of the abdominal canal, the crural hernia is by far the most frequent in women. A very striking distinction between the two species, lies in the diversity of their mode of production. The origin, indeed, of crural and inguinal hernia in the robust male and female, presents all the seeming difference of injury and predisposition. In the former the gut is suddenly projected as it were by a real rupture, in the latter it creeps slowly into notice by long and gradual distention.

The perpendicular pressure employed to reduce crural hernia is a matter of more importance than is commonly imagined. The taxis is generally esteemed one of those simple points of practice which are to be taken in the natural way. The section of a vein and the turns of a bandage require, even in our days, some share of education; but of simple manual pressure, all are supposed intuitively masters. The direction of pressure in the taxis is not less important than the inclination of the bistoury in the operation, and as accurate a knowledge of the local anatomy is essential for the one as for the other. In preparing for the last and greatest extremity it is common to overlook the means which might have prevented it. If the taxis could be always skilfully applied during the first hours of strangulation, the proposals of modern operators would be as little the subject of examination as the *punctum aureum*, or *the royal stitch.*

The direct descent of crural hernia rarely permits a permanent cure, an event to which the obliquity of the inguinal is certainly favourable. The adherence of the intestine to the sac is a comparatively rare occurrence in the irreducible crural hernia. It is produced by gradual pressure, and not by sudden violence; and hence we suppose is least liable to inflammation; and as it seldom attains to any great size, it can afford but a small surface for secretion.

"With regard to the seat of the strangulation in crural hernia, it will be found in three different situations.

"First, in the crural sheath, and semilunar edge of the fascia lata.

"Secondly, at the posterior edge of the crural arch.

"Thirdly, in the mouth of the hernial sac, and fascia which covers it.

"With respect to the first of these, it will be found that a portion of the crural sheath remains below the crural arch, which forms a strong circular band by which the hernial sac is surrounded. When the sac is drawn from the sheath in the male, the orifice by which it has passed will be found to be formed by two strong columns of fascia, one passing above, and the other below the sac, and meeting on the inner side at the posterior insertion of the external oblique muscle into the ligament of the pubis, and on the outer side at that part of the sheath which covers the crural vein. In the female these columns are not equally strong, still a portion of the sheath surrounds the hernial sac. If the hernia is large, it reaches to the semilunar edge of the fascia lata, and is compressed by it.

"The second place at which the stricture is found, and which requires division in large crural herniæ, is the posterior edge of the external oblique muscle and the facia transversalis, which pass before the mouth of the hernial sac, and which extending inwards to be inserted into the pubis, form also the inner boundary of the sac at this part, whilst a small process of fascia on the inner side of the vein forms its outer boundary. Thus then there are two seats of stricture, one the edge of the aperture in the crural sheath, and the other about half an inch above it, formed by the posterior edge of the external oblique muscle.

"The mouth of the hernial sac is the other seat of the stricture.

"This aperture is very generally small when compared with the size of the hernial sac, and being much pressed upon by the posterior edge of the crural arch it undergoes a slow process of inflammation, which thickens very much the fascia in which it is enclosed. If the sac is removed with the fascia which encloses it, its mouth will be found to form a dense and compact substance, which firmly resists any attempt to dilate it by pressure."

The femoral ligament of Mr. Hey, it will be seen, contributes in some cases to form the stricture. Since the sheath concurs to the same effect, we may conclude that Mr. Hey was unknowingly in the habit of dividing both at the same time. This gentleman's description, we regret to say, is not sufficiently precise to warrant an inference; or we should be led to conjecture, that the space which he describes as comprehended by the two layers of Poupart's ligament, was just that which separates the anterior from the posterior stricture; and thus, in those cases where the external was ineffectually dilated, "a small division of the most interior part proved sufficient." The transverse fascia fortifying the top of the sheath, receives some strong tendinous fibres from the external oblique, and would of itself, we think, frequently oppose the reduction of hernia, if the posterior edge were ever so freely divided.

It will be seen, by a reference to the plates, that the second seat of stricture, viz. the posterior edge, is situated somewhat higher, and nearer to the ilium, than the process described by Gimbernat. But, as our author very justly observes, a stricture cannot be locally defined in reference to a continuous edge or surface. The stricture exists in every part of it, and hence the crescentic duplicature is as much the seat of stricture as the posterior edge of which it is the insertion. Thus, if we speak of the seat of stricture, we must comprehend the posterior edge and duplicature under the general term of the third insertion, &c. if we speak of the part to be divided, we may define it to be any sub-division of this whole*.

We are rejoiced to find the most experienced modern practitioners joining issue upon the subject of an early operation. The number of lives sacrificed to previous methods, either from a want of confidence to operate, or a blind reliance upon palliatives, if it could be calculated, would scarcely be believed. Since the anatomical mysteries are unravelled, the path of the surgeon is cleared, and nothing, surely, but the most inexcusable ignorance or indecision could tempt him to prefer a course of doubt and danger, to one of ease and safety.

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* We are aware of the inaccuracy of Gimbernat's description of the stricture that bears his name, and believe many have fallen into his error. The fibres of the crescentic tendon are not curved inflections of the fibres of the posterior edge of the external oblique, but a distinct order arising more or less angularly from this edge. Still the connexion between them is such, that a division of either has the same effect in the relaxation of the whole.

The inducements to operate early, might be still more strongly represented; for, admitting the success of one of the long series of Prophylactics, the patient suffers less from such an operation, than from the pain of protracted manuism, added to the anxiety of his mind, lest it should prove ineffectual. Besides, when the operation is done early, it is done easily; the parts have undergone no recent changes, there can seldom be any necessity for opening the sac; and if there be, the danger is less of succeeding ileus. More than half the operation is still simple taxis. We have known perfect relief obtained in an early bubonocele, from the dilatation of the external aperture, through an incision of barely three inches long. This requires little more surgical skill than the manual operation. "At the time of writing this," says an experienced author, (to whose discriminative judgment, surgery is much indebted), "I have performed the operation thirty-five times, and have often had occasion to lament that I had performed it too late, but never that I had performed it too soon." Hey's Observat. in Surgery.

"The next part of the operation consists in the division of the stricture; for this purpose the finger is to be pushed gently into the sac, and the omentum and intestine separated from its anterior part, the probe-pointed bistory which I have recommended, which does not cut near its point*, is to be pushed into the crural sheath, at the anterior part of the sac, and the sheath is to be cut as far as the anterior edge of the crural arch, or Poupart's ligament. In a small hernia this division, which does not exceed half an inch, will be sufficient for the reduction of the parts. This was all that was done in Cases II. IV. and V. and the intestine was easily returned by this incision.

"But when the sheath has been thus divided, if the intestine, when slightly compressed, cannot readily be emptied, the finger must be passed at least half an inch higher, and then the posterior edge of the crural arch and the fascia that covers it will be felt, forming a sharp edge, strongly compressing the mouth of the hernial sac. To divide this edge, the knife must be carried within the stricture, and being inclined obliquely inwards and upwards at right angles with the crural arch, a cut may be very safely made in that direction sufficient for the purpose of liberating the intestine from pressure.

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* "I have used this instrument several times, and can now recommend it still more strongly than formerly, on account of its safety in not injuring the intestine within the abdomen.

"The two incisions which I have directed, being made from the interior of the sac, any stricture arising from the contraction of the sac itself, will be at the same time removed, and the protruded parts be thus completely liberated.

"If the hernia is large, and any pressure has been made on its contents by the semilunar edge of the fascia lata, the first incision will divide that edge."

We should have expressed our surprise at this unexpected deviation from the direction given us in the former part of the work, to dilate externally to the sac, if we had not always inclined to question the facility of its execution.

The very narrow neck of the crural hernial sac, compared with the size of its body and fundus, and the smallness of the aperture through which it passes, would a priori lead us to expect that it requires to be dilated before its contents can be returned. In the old and irreducible hernia, the parts have a tendency to overgrowth, as Arnaud aptly illustrates by Horace's fable of the weasel, and the sac and its contents soon became incorporated with each other. Where a hernia is suddenly strangulated upon its first appearance, as sometimes happens in the inguinal species, we consider the exterior dilatation an essential improvement, because it is easily practicable. Where, on the contrary, the hernia is of long standing, however recent the strangulation may be, we believe the attempt would be attended with almost insuperable difficulties.

Where th esheath causes the stricture, its morbid adhesion to the sac prevents the introduction of the knife between them; and where the strangulation is confined to the posterior edge, which, by the way, we much doubt if it ever is, the firmness of the natural connexion between it and the sheath, would equally foil the operator. We confess we should have been better pleased with the author's personal testimony of the difficulties attending Mr. Else's operation. A person not thoroughly acquainted with the structure of the parts, might have made the difficulties of which he complained. This circumstance is perhaps of itself an objection to the operation. From the recommendation given in the former part of his work, to leave an opening for the discharge of fœces in mortified intestine, our author substitutes the application of four ligatures, so as to shut the whole circumference of the bowel. First, because the opening left is prone to degenerate into an artificial anus. Secondly, because the food, if unhappily the ilium be the intestine under treatment, is not detained long enough to afford nourishment. Notwithstanding the favourable result of the experiments formerly detailed, which proved that an intestine may be returned into the abdomen, after being secured by ligatures, Mr. C. finds the process of ulceration so quick in parts highly inflamed, that he prefers to carry the intestine to the mouth of the sac, and there to fix it by its mesentery. The varieties of crural hernia, are:—First, the entire or partial rupture of the superficial fascia, that ties together the absorbent glands. Second, the extension of the sac, also entire or partial over the vein, and perhaps through the sheath, "so that the hernia turns before the vein, instead of descending by its side." The fascial production between the vein and absorbents, is by no means so true or so strong a septum as that between the blood vessels, and is sometimes wanting altogether; the cause of the above-mentioned variety. Third, the umbilical artery traversing the mouth of the sac from its outer side; and lastly, the pubal course of the obturator artery. In six out of twenty-one preparations in the possession of Mr. Cooper, and six out of thirteen belonging to Mr. Thomson, the epigastric and obturator artery arise by a common trunk; but it is only when the trunk is unusually long, that the obturator courses over the neck of the sac. The common origin of the two, we find to be very nearly as frequent as the distinct one, but we have very rarely observed much length of trunk. To provide for this, almost a circulus arteriosus, constitutes the essential difficulty of projecting an operation invariably safe for the crural hernia.

"When therefore this artery passes before to the sac (as in the case observed by Dr. Barclay), the arterial trunk common to it and to the epigastric is of unusual length; for when the trunk is short, the obturator passes behind the sac. A hernia thus situated is surrounded by blood-vessels, except at its posterior part, which might seem to render it advisable to deviate from the usual mode of operation to prevent this blood-vessel from being wounded. This however is not so liable to happen where the division of the stricture is made upwards, or a little upwards and inwards; for it will be found that the greatest distance between the artery and the hernial sac is at its anterior part. It is impossible to feel this artery before the introduction of the knife, for the finger cannot be passed behind the posterior edge of the crural arch, beyond which this artery is placed, until the stricture has been divided. The sac therefore is to be carefully divided anteriorly; but even supposing the artery to be wounded in the operation, it may be asked, what other direction of the wound would afford greater facility of tying the bleeding vessel? For by slitting up the crural arch, and drawing down the mouth of the hernial sac, the vessel would be brought into view, and might be secured."

The only precautionary mode of operating with which we are acquainted, is that proposed by Professor Thomson, of the University of Edinburgh.

Since the artery cannot be felt before, he recommends that it should be felt for behind the posterior edge of the crural arch; and if its inward march be ascertained, thrown back upon the leg of the director, while the bistoury is conveyed into its groove.

If this operation, which is of course liable to the same objections as that of Mr. Else, is invalidated by those objections, we do not hesitate to prefer that of Mr. Cooper, since it is infinitely less difficult of performance, and offers a fairer prospect of escaping the vessel than that of Gimbernat. Indeed admitting the variety, and the greatest distance between the artery and the hernial sac to be at its anterior part, it is hardly possible, with a knife such as our author recommends, if properly used, to produce mischief. Every operator of common caution, would press uniformly from the blunt point and edge as his knife advanced, until the sharp edge came in contact with the tendon; the instantaneous yielding of the stricture would sufficiently announce this event; and if he were even incautious enough to continue the dilatation, the strong probability is, that the length of its cutting edge would be insufficient to enable it in its *progress* to reach the artery. Mr. Cooper states, from experience some pointed objections to the operation of Gimbernat. These are its difficulty, from the depth and concealment of the part to be divided; the danger of cutting or tearing the intestine; its insufficiency when the tumour is large; and the greater danger of wounding the artery in the last-mentioned variety. To the objection of difficulty in reaching, and imprecision in dividing the stricture and parts about it, we readily accede; and this extends to the recovery of the bleeding vessel, as well as to the release of the sac. Wounds of the intestine, like those of the arteries in bleeding, are seldom, we believe, the fault of the operation: and if dilatation were practicable exterior to the sac, this objection would be done away.

The *efficacy* of the operation of Gimbernat, for reasons already given, we should have apprehended to be identically the same as attends the division of the posterior edge, since the alteration proposed refers to the place and direction of the division, and not to the stricture to be divided. It is not easy to conceive the division of the posterior, unaccompanied by that of the anterior stricture; but it is a very possible case that the surgeon shall do more than is required of him, nay, shall in some cases do mischief, from a persuasion that the reflected tendon is the seat of stricture, and a resolution in all cases to divide it. Cases illustrating the peculiar advantages of our author's practice, conclude this division of the subject.

In his anatomy of umbilical hernia, our author expressly asserts the completeness of the peritoneum behind the hole of the tendon, and the passage of the sac through the latter. It appears that in the only two cases which he has seen of deficiency of sacculus, the membrane had been partially absorbed or lacerated, so as to allow the protrusion of its contents, and threaten from this cause a double stricture. It is probable that similar appearances, less closely inspected, gave rise to the opinion so firmly maintained by Dionis, De la Faye, and others, that the peritoneum was in this species always ruptured. Another variety is the co-existence of two, or even of several distinct sacs. In small herniæ, a steel spring truss, on the principle of the inguinal, is found most efficient. Where the hole of the tendon is large, the section of an ivory ball, of diameter proportioned to the size of the tumour, is to be compressed by the pad. Small circular pieces of the emplast. adhes. so cut as to form a cone with its apex inwards, have been found efficacious in slight cases. In the infantile exomphalos, the smaller section of an ivory ball may be compressed by adhesive plaster, and for the irreducible hernia a saucer-like hollow truss is recommended to prevent increase. These are but slight variations in figure, and differ nothing in principle from the instruments described by Celsus, Scultetus, and others. The following operation, our readers will see, is intended to obviate the ill effects of a direct opening; and is, we think, preferable to the simple longitudinal or cruciform incision.

> "I have, therefore, in two instances performed the following operation. As the opening into the abdomen is placed towards the upper part of the tumour, I began the incision a little below it, that is, at the middle of the swelling, and extended it to its lowest part. I then made a second incision at the upper part of the first, and at right angles with it, so that the double incision was in the form of the letter T, the top of which crossed the middle of the tumour.

The integuments being thus divided, the angles of the incision were turned down, which exposed a considerable portion of the hernial sac."

In large and long irreducible exomphalos, the operation is rendered safer by leaving the sac unopened. In dilating the stricture of the sac, the chief danger to be apprehended, is that of wounding the intestine, and to prevent this accident, the tip of the finger, as Heister also has advised, should precede the knife, a precaution not less effectual and far more simple than the Bistouri caché of Le Dran. In cases of firm adhesion, we are advised to leave the intestine in the sac, and bring the edges of the skin carefully into contact. A selection of illustrative cases, which, we are persuaded, the most experienced practitioner cannot peruse without improvement, concludes the subiect of umbilical hernia.

A deficiency of the abdominal parietes giving rise to this disease in infancy, is stated in a letter from Dr. Hamilton, jun. of Edinburgh, to have occurred under his observation as often as twice annually for the last 17 years. We cannot quit this subject without adverting to the valuable hint furnished by Case XI. the truth of which we have often witnessed, viz. that a concomitant disease, not of itself mortal, will prevent the success of an operation, the result of which might have been otherwise favourable.

The ventral hernia, described by Celsus, is of unfrequent occurrence. It is seated usually in the lines of the abdominal tendons, below the plane of the umbilicus; and its causes are the dilatation of natural foramina transmitting vessels, congenite deficiencies, lacerations, and wounds of the muscles or their tendons. Beneath the superficial fascia a second is found filling the aperture in the dissection of the small hernia; the larger sac is covered only by the former.

Medical treatment is commonly more efficient in this than the other species, when strangulated.

The valvular incision is here recommended, and dilatation either upward or downward, according to the relative situation of the tumour and epigastric artery, which crosses the lower part of the linea semilunaris.

Garangeot, in his interesting memoir "sur plusieurs Hernies singulieres,*" narrates the case of a young student in surgery, who after two years of excessive suffering from a hernia of the stomach, produced by too forcibly expanding the chest in dancing, fortunately heard the exact detail of his symptoms in the school of that celebrated teacher, and obtained instant relief by the application of a truss. In this and similar cases, he observed the hernia to be produced by the side of the ensiform cartilage, through a triangular space formed between it and the sheath of the rectus muscle, and covered only by the aponeurosis of the external oblique.

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* Acad. de Chirurg. vol. 1.

Of the pudendal hernia, or that which descends between the vagina and ramus ischii, and forms an oblong tumour in the labium, traceable within the pelvis as far as the os uteri, we have not seen, and do not remember to have perused a case. Our author suggests the probability of its having been mistaken for the hernia foraminis ovalis.

Of the vaginal hernia we have cases recorded in the Acad. de Chirurgie, and elsewhere; but for a full and systematic account derived from clinical as well as morbid examination, we refer to the Observat. Anat. Patholog. of Sandifort. vol. I. Two examples only have met Mr. Cooper's observation. Of the perineal hernia the most impressive case is that which occurred to Mr. Bromfield of St. George's Hospital, which is here transcribed. We have likewise a description of it in Camper's Demonstrat. Anat. Pathol. Of this and the vaginal species, our author has favoured us with a concise anatomical history and plates.

Hernia foraminis ovalis. Of this rare disease a specimen is preserved in the museum of St. Thomas's Hospital. Garangeot's Memoir furnishes the fullest account of it with which we are acquainted. The operation, if it became necessary, would present serious difficulties, and should the obturator rise in common with the epigastric artery, the dilatation inwards, which alone would be safe under common circumstances, would almost inevitably divide it. On the subject of cystic hernia first observed by Dom. Sala, our author is silent, as he has had no opportunity of examining the morbid anatomy, and feels dissatisfied with the details of others. Notwithstanding the number of these details, we shall look impatiently for his appendix.

From the variation in the state of the tumour, as the bladder is full or empty, the diagnosis of the cystic hernia is peculiarly plain*. We refer the curious inquirer to the scarce treatises of Gunzius and Hoin for information on this, and other rare forms of the disease.

A very interesting case of ischiatic hernia is given upon the authority of Dr. Jones of Barbadoes. The anatomy is well explained by plates, exhibiting a double view of the parts. The operation we cheerfully resign into the hands of Mr.

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* Pipelet le Jeune (Acad. de Chirurg. vol. 4.) mentions a cystic hernia in perineo, in the male, and refers to several cases of its occurrence in the female.

Cooper, and the description of it to the figurative genius of an author beyond the Tweed.

Of the phrenic hernia, our readers may recollect two dissections formerly published by our author in the Medical Records and Researches, which are here extracted. The causes are the dilatation of natural apertures, mal-formation, and wounds of the diaphragm. The second is the most frequent. Morgagni furnishes an instance of the first. Dr. Macauley's memoir in the Medical Observations and Enquiries, and the cases just alluded to are examples of the second; and the third is derived from the case of a ship-painter who died in Guy's Hospital in 1804. It is totally beyond the reach of art, and fortunately not very common. Cases however may be found in the writings of Hildanus, Paré, Petit, Schenck, and others. A lapse of the small intestines between the layers of the mesentery, which our author met with in the dissecting room, he terms, with sufficient propriety, the mesenteric hernia. We believe the case is now first described.

Extensive involutions and entanglements of the intestine are sometimes seen in the subject; and perhaps this variety of disease, from its rare strangulation, has escaped notice. A description and plate of a mesocolic hernia are likewise presented. As the sacs of these herniæ are not enveloped by tendons, they are probably never the subject of stricture. Unfortunately the history of both is wanting. Several cases are lastly related of strangulation within the abdomen from three causes, First, apertures in the omentum, mesentery, or mesocolon. Secondly, adhesions from inflammation leaving an aperture. Thirdly, membraneous bands forming in the mouths of sacculi elongated by the protrusion and frequent returns of viscera.

The concluding plate presents sketches of these appearances, and two views of the intestine of a dog united after longitudinal division, constituting an exception to the general results obtained by Professor Thomson. We have no hesitation to attribute the difference of effect which must be allowed to prevail in these cases, to the division of those circular fibres in the longitudinal wound, which in the transverse are only separated. The circular, besides being the strongest and most numerous, are situated most internally. The peristaltic motion requires the regular alternate contraction of both. In the transverse division the external and weaker are cut across; in the longitudinal, the internal and stronger. The suture may be used with equal impunity upon the inflamed as upon the sound intestine.

A case of double congenital inguinal, and crural hernia, is introduced in the form of a communication from Mr. Allan Burns, the diligent anatomist of Glasgow. The peculiarity of the former consists in the premature separation of the pillars of the external abdominal aperture, we suppose from a relaxation or total deficiency of the connecting fibres. We have a case of this description now under observation. The tumour of several years standing, is confined between the apertures of the inguinal canal, and instead of advancing through the lower it has dilated the tendon into a large pouch, and lost the visible character of hernia. In this case, which we believe was first noticed by Petit, the tumour takes an unusual inclination towards the spine of the ilium, and is upon the ilial side of the epigastric artery. From the following paragraph it is evident, that Mr. Burns had observed the fascia propria, and that he explains it by the protrusion of the fascia (or septum, as he calls it,) of the crural hole How the sac should derive an investiture from the duplicature of Gimbernat, we are utterly unable to conceive, and fear that even the sagacity of Mr. Burns would be puzzled to demonstrate.

"With regard to the crural hernia, we found a peculiar disposition of Gimbernat's duplicature; the formation was not however exactly alike on both sides. On the left of which I have sent a drawing, the crescentic duplicature was pushed out so as to form an enclose for the peritoneal herniary sac. In order to understand how this appearance was produced, we have to recollect in some cases a septum is stretched across the crural aperture, being merely a small perforation in the centre. When pressure is applied to this septum, it is protruded, assumes the form of a capsule containing the herniary sac, and has a small opening in its extreme point. Over the upper and outer edge of the protruded duplicature lay the falciform process of the fascia vera; but in this instance it is obvious that the fascia might have been divided, without in any degree removing the cause of strangulation."

We must here close our analysis of this important work, to the standard matter of which it will be found more easy to append a criticism, than to oppose a fact.

It has thrown a new and brilliant light upon the tangible pathology of hernia. The abdominal ring, as it is called, was the ultima Thulé of our forefathers' excursions in the inguinal region*; and a vaguely defined space comprised between the ilium and pubes, was almost all the anatomy of our most eminent writers. Some spoke with modesty and a becoming caution, and hinted, obscurely indeed, the concern of more parts in the strangulation; others affected authority, and blazoned their ignorance by a captious and dogmatical pertness*. In no operation is the embarrassment of operators more conspicuous, and surely in none more alarming. Often have we seen the surgeon scratch and niggle with his scalpel at the distance of a finger's breadth from the intestine, and like the traveller who has lost his way, "seem afraid to go forward least he should go wrong." The caution of ignorance is as little worthy of confidence as the boldness of empiricism. The supposed thickening of the sac, and its divisibility into layers, the feuillets which Duverney mistook for adipose cells, and Garangeot for the cellular coat of the peritoneum, are almost invariably mentioned by operators; but the idea that they are distinct fasciæ compacted by disease, seems never to have been conceived. The general doctrines of causes and symptoms, are detailed in the former part of the work, and they are delineated with much strength of character and accuracy of discrimination. The plates drawn for the most part by Kirtland, are engraved by Heath, in the style peculiar to the latter artist. Our metropolis, we lament to say, with all its comprehensiveness of excellence, does not boast a master in anatomical delineation. The works of Camper, Scarpa, and Soemmering, are at once our admiration and our shame.

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* Our well known obligations to the ingenious Camper are of a later date.

We know of no example that better deserves imitation than that of Mr. Cooper. It is only by concentrating our observation that we can hope to add to the store of facts, which experience has already accumulated, Such insulated labours cannot be too much encouraged, nor too highly appretiated.—The results of speculative efforts are like the ignis fatuus which gleams only for a time, and leads the traveller astray; but the effects of these practical investigations may be compared to the polar light which guides the mariner from the shoals and rocks, and shines with undeluding splendor.

The history of the disease whose seat, symptoms and treatment arehere delineated from the life, may ere long be supplied by our more erudite northern neighbours; and though the opportunities of literary labour lie within a smaller compass and are of more general attainment, than the opportunities of an equally extensive observation; they are not less deserving of the protection and encouragement of all, who desire to ennoble the profession that they are compelled to cultivate.

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* Vid. A Treatise on Ruptures, Chap. I. 1783.

ART. IV. *A Treatise on Insanity, in which are contained the Principles of a new and more practical nosology of maniacal Disorders than has yet been offered to the Public, exemplified by numerous and accurate historical Relations of Cases from the Author's public and private Practice: with Plates illustrative of the Craniology of Maniacs and Ideots.* By P H. Pinel, Professor of the School of Medicine at Paris, Senior Physician to the Female National Asylum la Salpêtrière, late Physician to the Asylum de Bicêtre, and Member of many learned Societies. Translated from the French, By D. D. Davis, M. D. Physician to the Sheffield General Infirmary. 8vo. pp. 288. London, Cadell and Davies, 1806.

IF any one will take the trouble to read the most esteemed works on mental insanity, we have no doubt he will think with us, that the treatises on this subject are more than commonly uninstructive, and that when arrived at the conclusion of his task, he knew little more than when he began it. Whatever may be our opinion of the nature of this disease, whether we believe it to originate in a mental or a corporeal affection, it is still true that the present subject is partly medical and partly metaphysical, and that even if we could clearly ascertain how far the bodily functions are concerned in the derangement of the mind, a no less important question would still demand solution, What change does insanity produce in the faculties of the understanding? or, in other words, What is the difference between a sane and an insane mind? The mental may be the effect of the bodily affection, and yet the mind may open the fairest track to the disease of the body. It is upon the latter question that our books afford the least instruction. A multitude of cases are upon record narrated with the utmost minuteness; but we look in vain for a clear, precise, and satisfactory analysis of that mental affection under which the patient labours. Sometimes we find an attempt to supply this defect by bad metaphysics, and sometimes are modestly told by the author that such investigations are useless. Unfortunately a rare concurrence of circumstances is necessary to the successful investigation of the diseases under consideration.

Those who superintend the management of the insane are seldom or never fitted either by the natural faculties of their minds, or the favourite line of their studies, to distinguish the essential features of any mental disease; whilst those who have attained more than ordinary aptness at seizing the subtle forms of intellectual peculiarity, are seldom thrown into a situation where they can apply this power to the extension of our knowledge. Those who have the faculty want the opportunity to observe, and those who have the opportunity want the faculty. An examination of the most probable sources of information, *viz.* the medical and metaphysical writers, leads to this short conclusion: that the physicians were dull, and the metaphysicians ignorant.

There is another cause, no less important, of the scantiness of our knowledge concerning mental derangement. The clearest source of facts relating to the nature of mind, is self-observation. But during the existence of insanity the madman is incapable of self-observation; he cannot look within himself to analyze and explain the difference between the state of his mind now and when in health. The most valuable source of information on this subject is taken away, and we can only gather our knowledge by observing the actions and conversation of our patients, and tracing them through several steps of inference to their proper sources in the understanding. These we believe are some of the causes why the books which treat of mental derangement are more than usually empty. Authors have attempted to give a definition of insanity, as if it was an uniform disease, or as if all its varieties could be traced to one essential hallucination. Some think that it consists in an unnatural association of ideas; some in a præternatural strength of imagination; others in an extraordinary susceptibility to passion, either of pride, rage, or grief. The truth is that the unnatural state of the mind, called insanity, varies in different individuals, and that any opinion drawn from a few cases, will not be found universally applicable. Insanity is far from being a uniform disease. It is merely a term for a disordered state of the mind, which is marked by one set of symptoms in one case, and by others in another; and if we attempted to give a completely comprehensive definition of mental disease, it would be precisely like the definitions of bodily disease with which Scotch professors make their pupils gape, and which if analyzed, will be found to mean little more *than a state differing from health*. Before we proceed to examine the volume which lies before us, we cannot refrain from attempting to point out the most common errors, and to offer a few hints to future investigators in this field of observation. Our readers must not expect to receive much light from us. The subject is naturally a difficult one, and as we have had few opportunities for observation, and suspect that we are not gifted with unusual sagacity, we cannot hope that our feeble taper shall in any degree dissipate a gloom which has shrunk in so trifling a degree before far stronger illuminations.

A maniac, during the effervescence of his disease, exhibits a curious specimen of incoherent discourse.

It is commonly supposed, therefore, that at this period his ideas are unnaturally associated. If you go into the cell of a maniac at this time you will hear him talking with the utmost volubility, and passing from one subject to another without any obvious connexion. The most probable reason for this apparent incoherency, is not that his ideas are unnaturally associated, but that his discourse presents you with only some of the thoughts which are passing through his mind: these appear unconnected because we are not in possession of the connecting links. The probability of this will easily be perceived by those who are accustomed to observe the transition of their own thoughts, particularly the phenomena of reveries or waking dreams. In this state of the fancy we begin by thinking of a particular subject, but by degrees the mind wanders from one thing to another until we stop, and are surprised how the first idea should have led to the last. Our curiosity induces us to trace back our steps one by one, from the thought with which we concluded to that with which we commenced our rambling, and perceiving the steps by which we were led from one to the other, we no longer wonder at the transition. The application of this to the incoherency of madness is obvious. In this state it is probable that ideas hurry through the mind with such uncommon rapidity, that although the maniac talks fast he thinks faster. He utters only some of the thoughts which flit across his mind, and we conclude they are unconnected in the usual way, because, as before observed, we are not in possession of the links by which they adhere. The remarkable feature, therefore, of this form of the disease is not an unnatural association but an unnatural rapidity of ideas.

As we have already said, mental insanity is far from a uniform disease; and we suspect, that if the functions of the mind were as clearly and fully understood as those of the body, and it was as easy to collect the symptoms in a case of mental as in one of corporeal disease, we should find the former subject to much greater variety than is commonly suspected. In some cases of mental derangement, the disease would at first sight appear to consist in a preternatural susceptibility to sorrowful emotions; but we believe further examination will induce a modification of this opinion. We allude to hypochondriacs, melancholics from love or religion, or other causes. Among the endless variety of characters which the world contains, there is a set of men, who have not yet acquired the epithet of madmen, but whose peculiarity of mind differs, we suspect, in nothing but in degree from the kind of insanity to which we allude. These characters are commonly called low-spirited, gloomy, desponding fellows. During an interval of occupation, when the mind is free to range where it pleases, they are constantly painting their future lives with a pencil dipped in black. Aware that they possess certain resources of money, knowledge, and patronage, they view their present situation in the same light with the most cheerful of their companions. But the character of the man, the extent of his resources, and the usual conduct of the world being given to find his *future* lot, he commences the calculation with the same assumptions as his friends, and differs from them in the conclusion. They deduce success; he misfortune: and the consequence is, that he becomes a frequent prey to those sorrowful apprehensions and gloomy emotions, which want only strength and permanence to constitute one species of mental insanity. This peculiarity of mind cannot be said to be a defect of judgment, not only because the above, unlike a mathematical problem, admits of various solutions by men of equal judgment, but because the kind of characters to which we allude, reason upon other subjects like other men. It cannot be said to be a disease of passion, because the emotions to which he is so often a prey, are the natural effects, both in kind and degree, of the ideas by which he is haunted. Every one, who credulously looked forward to the same lot, would be oppressed by the same feelings as the low-spirited man. This peculiarity of mind, when arising to an excessive degree, constitutes that simple form of hypochondriacism, which is not attended by any of the absurd conceptions sometimes conjoined with this state of mental derangement. The disease is not of the judgment or the passions, but of the fancy; and there exists this important distinction between this form of insanity and that in which the patient fancies that his legs are of straw, or his back-bone a barometer; that in the one case the hallucination relates to future, and in the other to present facts; in one to probabilities, in the other to certainties.

The form of insanity which appears to us the most difficult to explain is that which consists in some ridiculous hallucination. One is emperor of Morocco, another hast lost his own head and got another in its stead; one obstinately retains his urine for fear of deluging the world; another believes that his backside is made of glass, and flies into a violent rage with his physician for recommending him to ride on horseback. Notwithstanding the apparent absurdity of these conceptions, it is unfair to suppose that the madman has no better reason for his belief than those which we perceive. If we had complete information of what had passed and was passing in his mind, we might be possibly led to acknowledge that seeing what he had seen, and hearing what he had heard, the apparently absurd conception had been formed without any dereliction of common sense; that the disease was in the imagination not in the judgment, and that the evidence which he possessed being given, he proceeded to form his conclusion in the most logical manner. We may even go farther and say that it cannot consist in a defect of judgment, for excepting on one unfortunate subject, the maniac frequently talks with pertinency and even acuteness. The passions are as little concerned in the disease, for frequently it is marked only by a hallucination which leaves the mind in a state of ease and placidity. It can only therefore consist in a disordered fancy, although from the natural obstacles to the investigation, it may be difficult to ascertain precisely what this disorder is. By fancy, we mean that faculty of the mind by which ideas are called up.

Fancy is the magic lanthorn, ideas the figures, and the perceptive faculty the white sheet upon which they fall. The fancy may be diseased in three ways, by representing ideas either with unnatural rapidity, or unnatural association, or unnatural vividness. The two former we have already illustrated; about the latter we have a few words to say.—Among the recorded cases of mental derangement we shall find enough of evidence to satisfy us that in this disease, ideas sometimes become as vivid and distinct as sensations, or, to speak in popular language, a man thinks he hears or sees what he only imagines. Mr. Haslam tells us that several of his patients declared that they had seen the devil, and minutely described his horns, cloven feet, long tail and fiery eyes.

If then a madman should think he saw a supernatural being, who told him that his relations had conspired against his life, or that he was the wandering Jew; (and what man of common credulity might not be brought to believe it?) a multitude of the most absurd conceptions might spring from such an origin. But there is another way in which preternaturally vivid ideas may give rise to hallucination. The memory represents scenes which have been witnessed more fully and distinctly than those which have been read of. A man who had been present at the battle of Narva, would at an after period have in his mind a much more minute and vivid picture of the scene than one whose information was derived only from history. Now a madman who had been reading the history of Charles XII. of Sweden, and had pondered on the events of that interesting reign, might acquire so full and distinct a picture of the battle to which we have alluded, as to believe that he had been present at it, and by a little mis-association might transform him selfinhis own belief into that victorious monarch. Unnaturally vivid ideas, may probably in various other ways give rise to those ridiculous suppositions, which so often form the principal feature of mental derangement. In the book which we are about to examine, some cases are related where the most trifling incidents excited paroxysms of rage without any defect of judgment or error of imagination. If the observation of M. Pinel may be trusted for a fact of this kind, we must be led to conclude, as has been conjectured, that the passions are sometimes the seats of insanity, and that these cases are neither more nor less than specimens of preternatural susceptibility to the emotion of rage, or in other words, ill-temper rising to such an excess as to constitute madness. It will be prudent here to put a stop to our speculations. The subject requires more than common faculties and opportunities for observation. We can wish for no more favourable coincidence to the elucidation of the present subject, than that a Hartley should superintend a mad-house; and we have little doubt that so much light would soon be thrown upon the morbid affections of the mind, that we should understand one another as well when speaking of a diseased fancy and a diseased passion, as we now do when speaking about a diseased liver or a diseased stomach.

During the second and third years of the French Republic, the author of the volume which we are about to examine was appointed physician to the Asylum de Bicetre, an institution which is partly dedicated to the reception of the insane. For five years previous to this time he regularly attended another and similar institution. During this time the number of cases which came under the observation of M. Pinel appears to have been considerable, and the volume before us is the fruit of his assiduity. What is the quality of this fruit will appear in the sequel.

M. Pinel is a diffuse writer, and nothing is easier than to put in a few lines what he puts in a few pages. We shall endeavour to present our readers with the most valuable information which we can extract from his book.

The most common form of insanity is the intermitting, or that in which the paroxysms of the disease are divided by lucid intervals. The paroxysms commonly begin immediately after the summer solstice, continue with more or less violence during the heat of summer, and terminate towards the decline of autumn. The accession of the paroxysm, however, is far from being regular in time. Three cases are mentioned in which they returned at the approach of winter, and the remissions and exacerbations corresponded with the changes of the weather from mildness to severe cold. Some cases seem to be affected not in the slightest degree by variation of temperature or any other obvious causes, and these cases are the most difficult of cure. The precursory symptoms of a maniacal paroxysm are detailed with great minuteness, and are sufficiently condensed to admit quotation.

" The patient complains of a sense of tightness in the region of the stomach, want of appetite, obstinate costiveness, and a sensation of heat in the bowels which obtains a temporary relief, from copious draughts of cooling liquids. He is subject to a kind of uneasiness which he cannot describe nor account for; experiences a degree of fear that sometimes amounts to terror, and feels either little disposition or absolute incapacity to sleep. Soon after, incoherence and incongruity of ideas are betrayed in his outward conduct, by unusual gestures and by extraordinary changes in the expression and movements of his countenance. He generally holds his head erect, and fixes his eyes and attention upon the heavens. He speaks with a deep hollow voice; walks with a quick and precipitate step; and stops suddenly, as if arrested by the most interesting and profound contemplations. Some maniacs are remarkable for great humour and merriment, which they express by fits of loud and immoderate laughter. There are others, as if nature delighted in contrast, whose taciturnity is perpetual, who express their afflictions by tears, or who sink without a tear under the distressing influence of solitary anxiety. The eyes, in some species of insanity, suddenly appearing red and sparkling, the cheeks flushed, with a vigorous and rapid action of the muscles of the countenance, accompanied by an exuberant loquacity, are certain presages of an approaching paroxysm; and point out the urgent necessity of strict confinement. I remember a maniac who suddenly spoke with great volubility, laughed immoderately, and then shed a torrent of tears. Experience had taught me the value of a speedy seclusion; for his paroxysms were extremely violent, and he broke into a thousand pieces whatever fell in his way.

"Dreams of extacy and visions of heavenly pleasure are the ordinary preludes to paroxysms of maniacal devotion:—as those of unfortunate love are preceded by similar interruptions of sound and healthful sleep. The beloved object appears under the form of an exquisite beauty, with every other advantage, greatly exaggerated by the magic power of fancy. But the too happy dreamer, after an interval of more or less continuance of reason and calmness, awakes once more the noisy, the disconsolate, and the furious maniac."

M. Pinel contends that insanity is a disease of mind not of body. This opinion rests wholly upon two arguments; the one is, that insanity, if curable at all, is curable by what he calls a moral regimen; the other, that in maniacs, there exists no visible organic change in the brain. For our parts we do not think that observation has yet supplied us with sufficient data to found any very decisive opinion upon. It is well known that the dissections of Mr. Haslam, the apothecary of Bethlem Hospital, are decidedly opposed to the statement of our author. Upon examining the heads of those who died of insanity, he almost always found some unnatural appearances, of which the most common were, congestion of blood in the pia mater and veins of the ventricles, loose adhesion of the pericranium to the skull and opacity, thickening and sometimes inflammation of the tunica arachnoides; in the ventricles there was sometimes more water than usual and sometimes none at all.

Analogy rather tends to prove that the derangement of the mind arises from that of some part of the body. Dyspepsia and chronic hepatitis often produce uncommon depression of spirits, frequently approaching hypochondriasis; during the low delirium of typhus, the patient will fancy he sees objects which are seen by no other person. In cases of paralysis also, we have heard the patient complain of strange persons being in the room at a time when no one is present but the nurse or the friends.

We have heard of a man subject to epilepsy, who before the fit, always saw a little old woman in a blue cloak and sharp-pointed cap enter the room with a crutch in her hand; she approached him and knocked him on the head with the crutch, when he instantly fell down senseless. All these are undeniable instances of mental deception arising from bodily disease, and go some way to prove that that degree of mental deception which constitutes insanity, may also arise from disorganisation or morbid action in some part of the body. In a former part of this article we have expressed our opinion upon the share which a diseased imagination has in the production of insanity. The following are curious and interesting illustrations of this position.

"I have frequently stopt at the chamber door of a literary gentleman, who, during his paroxysms, appeared to soar above the mediocrity of intellect which was habitual to him, solely to admire his newly acquired powers of eloquence. He declaimed upon the events of the revolution with all the force, the dignity, and the purity of language that the very interesting subject could admit of. At other times, he was a man of very ordinary abilities.*

"It is well known, that certain professions conduce more than others to insanity, which are chiefly those in which the imagination is unceasingly or ardently engaged, and not moderated in its excitement by the exercise of those functions of the understanding which are more susceptible of satiety and fatigue. In consulting the registers of Bicetre, we find many priests and monks, as well as country people, terrified into this condition by the anticipation of hell torments: many artists, painters, sculptors and musicians: some poets extatized by their own productions: a great number of advocates and attorneys. But there are no instances of persons whose professions require the habitual exercise of the judging faculty; not one naturalist, nor a physician, nor a chemist, and, for the best reason in the world, not one geometrician.

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* A madman, that was cured by the celebrated Dr. Willis, has given us the following account of his own case:

"I always expected with impatience the accession of the paroxysms; since I enjoyed during their presence a high degree of pleasure. They lasted ten or twelve hours. Every thing appeared easy to me. No obstacles presented themselves, either in theory or practice. My memory acquired all of a sudden a singular degree of perfection. Long passages of Latin authors recurred to my mind. In general I have great difficulty in finding rhythemical terminations; but then I could write in verse with as much facility as in prose. I was cunning, malicious, and fertile in all kinds of expedients."

It is commonly believed that maniacs possess extraordinary strength and great power of resisting hunger and cold; this however is far from being universally true. M. Pinel mentions some cases which were attended by great muscular debility, great sensibility to cold, a voracious appetite, and inability to bear short privations of food.

"Paroxysms of insanity, which continue with more or less violence during the whole of summer and the greatest part of autumn, seldom fail to induce a considerable degree of exhaustion. The high excitement, so lately characteristic of active mania, is now exchanged for a languor approaching to syncope, a general sense of lassitude, extreme confusion of ideas, and in some instances a state of stupor and insensibility, or rather a gloomy moroseness, accompanied by a most profound melancholy. The patient lies in bed extended and motionless, without a wish or an effort to leave it. His looks are altered and his pulse is feeble and depressed. It now becomes the duty of the keeper to redouble his vigilance. If the weather be very cold, there is the utmost danger to be apprehended. Artificial heat must be speedily applied, and what is generated by the natural processes of the system, must be confined by an additional number of blankets or other clothing. Cordials and tonics are particularly indicated. Should this sudden change take place in the night, it may be fatal before morning:—a circumstance which renders it a duty of serious and important obligation upon the directors of lunatic asylums, to furnish their respective institutions with faithful and zealous servants, who shall see the patients frequently in the course of the night, especially at the commencement of the winter season. Great attention is paid to this branch of domestic police at the Asylum de Bicetre.

"To meet the well-ascertained wants of the Hospital de Bicetre, it was determined, by the Constituent Assembly, to increase the allowance of bread to one kilogramme daily. For the two succeeding years, I witnessed, with great satisfaction, the operation of that salutary measure. I then ceased to be physician to that hospital. But, during one of my friendly visits, (4th Brumair, year 4,) which I occasionally paid to my old insane acquaintances, I learned that the usual allowance of bread had been reduced to seven hectogrammes and a half *

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oz. dwts. gr.
* A hectogramme is equal to 3 : 4 : 8·40. Troy. T.

per day. A great number of the old convalescents had relapsed to a state of raving madness, and were complaining, loudly and bitterly, that they were about to be starved to death. But this system of retrenching was afterwards carried to still greater lengths, the allowance being gradually reduced to five, four, three and even to two hectogrammes of bread, with a small supplement of biscuit, which frequently was far from being of a good quality. The consequences were such as could not have escaped attention. Upon inquiring into the state of the institution, it appeared, that in the short space of two months (Pluviôse and Ventôse, year 4,) the total number of deaths, in the lunatic department alone, had been twenty-nine, while during the whole of the year 2, twenty-seven only died."

Insanity after continuing for a longer or shorter period without relief, commonly terminates in fatuity, especially when the disease had been treated by venesection and starvation. This state of mind, or rather this destruction of all mind, is almost always incurable, and the pitiable victim wanders about a walking corpse. Sometimes however young persons, after having remained in a state of complete fatuity for months or even years, are suddenly seized by a paroxysm of insanity which restores them to reason.

"This state of degradation and nihility, is strongly marked in the Cretins of Switzerland. Those monuments of imperfect reproduction of the human species, exhibit, from their earliest years, unequivocal indications of their future destiny. A swelling of the throat, about the size of a nut; a tumescent distortion of the face; the diminutive size of their heads and hands; defective sensibility to the variations of the atmosphere; difficulty in sucking, apparently from an imperfection of the instinct; general insensibility; extremely slow and incomplete development of the power of articulating sounds, confined as it is to the enunciation of vowels; clumsiness of all their movements in proportion to the increasing size of their bodies, are characteristics of their earliest infancy. At ten or twelve years of age, their intellectual powers are so little developed, that they are unable to convey the food to their mouth or to masticate it; and their mechanical existence is preserved by thrusting the aliment down their throats. As they grow up, their efforts to walk are feeble and clumsy; and if constrained to move, they never do it cheerfully, but always with an air of surly dissatisfaction, which maternal tenderness alone can tolerate. Their diminutive heads are always depressed on the crown and the temples, and the occiput is less than ordinarily protuberant. Their eyes are small, sometimes sunk, sometimes prominent, but always fixed and dull. Their chests are broad and flattened; their fingers long and delicate, with the articulations of the joints indistinctly marked. The soles of their feet are broad, and the foot itself generally distorted. At the age of puberty, which arrives at a very late period, the organs of generation are developed to an enormous size: hence their extreme salacity and propensity to onanism. It is at this period, that they begin to walk; but even then, their powers of locomotion are very limited, and are roused to action only by the desire of gratifying hunger, or of basking before the fire, or in the sun. Their bed is the end of a long and laborious journey to them, to which they retire with pendant arms and tottering steps. In walking, they keep their eyes fixed on their object, and pursue the rout most familar to them, regardless of obstacles and dangers. Arrived at their full growth, which is generally from thirteen to sixteen decimetres, their skin becomes brown; their sensibility continues blunt, which is indicated by great indifference to the action of cold and heat, or even to blows and wounds. They are generally both deaf and dumb. The strongest and most pungent odours scarcely affect them. I know a Cretin, who devours raw onions, and even charcoal with great avidity. A striking proof of the coarseness and imperfect development of the organ of taste. Their organs of sight and feeling are equally limited in their operation. Of affections, they seem wholly destitute: they discover no signs of gratitude for kindnesses shewn to them, nor any attachment to their nearest relatives. In a word, they appear almost totally insensible to objects which excite the interest of mankind, and even to the wants which more immediately affect the functions of life."

As insanity arises from ideas being unnaturally rapid and distinct, so idiocy arises from their being unnaturally slow and feeble.

Sensations, ideas, emotions, and all the consequent voluntary actions, are indistinct and imperfect, because the brain wants that natural excitability which is necessary to their perfect production.

When idiocy follows insanity it arises from an exhaustion of sensorial irritability, produced by the previous mental excitement which attends a paroxysm of madness, or to use the almost excluded phraseology of Brown, it is indirect debility of the brain.

The moral treatment, as it is here denominated, of M. Pinel, differs little from that of our most eminent writers on insanity. The remedies of this class upon which our author relies, are authority, persuasion, dexterity, and if necessary, confinement, darkness and water-gruel. M. Pinel, however, dislikes any long continued privation of food. All modes of debilitating, aggravate the symptoms of the disease or increase the probability of supervening idiocy. In melancholia he recommends entertaining books, cheerful conversation, and amusing scenes.

Employment of mind and body is recommended in every kind of insanity, when the patients can be trusted out of confinement.

"A principal hospital of Spain, presents in this respect an excellent example for our imitation. The maniacs, capable of working, are distributed every morning into separate parties. An overlooker is appoined for each class, who apportions to them all, individually, their respective employments, directs their exertions, and watches over their conduct. The whole day is thus occupied in salutary and refreshing exercises, which are interrupted only by short intervals of rest and relaxation. The fatigues of the day prepare the labourers for sleep and repose during the night. Hence it happens, that those whose condition does not place them above the necessity of submission to toil and labour, are almost always cured; whilst the grandee, who would think himself degraded by any exercises of this description, is generally incurable."

Removal from home, and seclusion from visitors, is recommended by M. Pinel, as well as by every writer of eminence. It is a fact well known to those who superintend mad-houses, that patients are seldom or never cured at home, and that even after they have been convalescent for some time, a return to their families has produced a return of the disease. This is not difficult of explanation. That unnatural action of the mind which constitutes the disease, becomes associated with a multitude of objects by which the patient is surrounded; these, as often as they stimulate the mind, produce a recurrence of the morbid train of thought; and removal from these, removes one of the causes which tend to perpetuate the disease. Besides, maniacs are often excessively disposed to rage, and men even in their sound senses experience less irritation of temper from strangers, than from familiars. This arises partly from a sense of shame and decorum which counteracts the rising feelings of wrath, but in a considerable degree from another cause. The same provocation from a stranger always produces less anger than from an intimate. We soon become acquainted with the little weaknesses, absurdities, and provoking qualities of our fellow inmates, companions in the same eternal round of breakfast, dinner, tea and supper. Whenever they offend us, the recollection of former offences rise by association in our minds, and contribute to increase the consequent degree of anger. On the contrary when a stranger offends us, the offence of the moment is the only cause of irritation, for there are no associated ideas by which it can be augmented.

The last section of the volume discusses the medical treatment of insanity. To idiots, bodily occupation and generous living are recommended. This treatment is useful in all cases of fatuity, but especially in those which are brought on by excessive evacuations; here it may possibly produce a fit of madness, of which the stimulus may rouse the torpid faculties of mind. Those cases of mania which are combined with epilepsy are almost always incurable. M. Pinel recommends that they should be separated from other patients, as the sight of their convulsions awakens either the terror or anger of the spectators. In the latter case they will approach the convulsed person, and attack him with furious blows. Melancholia should be treated with tonics, and attention be paid to the stomach and bowels. Some cases are related in which melancholia followed the disappearance of cutaneous eruptions, and was removed by a seton in the neck. Similar treatment is recommended in similar cases.

The extent to which venesection is applicable is thus stated by the author:

"The blood of maniacs is sometimes so lavishly spilled, and with so little discernment, as to render it doubtful whether the patient or his physician has the best claim to the appellation of a madman. This reflection naturally suggests itself upon seeing many a victim of medical presumption reduced by the depleting system of treatment to a state of extreme debility or absolute ideotism. At the same time, I do not wish to be understood as altogether proscribing the use of the lancet in this formidable disorder. My intention is solely to depreciate its abuse. A young Swiss, whose case is detailed by Van Swieten, took a sea voyage during a very hot season, and, when he came on shore, drank wine to great excess. It is not at all to be wondered at, that his cure was readily effected by repeated bleeding. Insanity, consequent upon the suppression of periodical or habitual discharges of blood, will doubtless frequently yield to an artificial evacuation of the same fluid, procured either by general venesection or topically by leeches and cupping. A paroxysm of mania is sometimes preceded by symptoms of its approaches which cannot be mistaken; such as heightened complexion, wildness and prominence of the eyes, exuberant loquacity. In such cases, the experience of hospitals authorizes the free use of the lancet. It is a well established fact, that paroxysms of madness thus anticipated, are in many instances prevented by a copious bleeding. On the other hand, I feel it my duty to abstain from this practice after the actual explosion of a paroxysm of irregular periodical insanity, as I am confirmed in the opinion that nature alone is adequate in this stage and form of the disorder to the performance of a solid and permanent cure. It frequently happens, that bleeding, practised as it is, without rule or bounds, is found to exasperate the

complaints

complaint, and to cause periodical and curable mania to degenerate into dementia or ideotism. In melancholia, whether simple, or complicated with hypochondriasis, bleeding is still less to be recommended. The character of that disorder is dejection. Its appropriate remedies are tonics.—Further experiments on this subject are only justifiable in the most hopeless and obstinate forms of insanity: such as regular periodical mania, inveterate continued madness, and madness complicated with epilepsy, or marked by a disposition to apoplexy. Out of fourteen patients who died at Charenton, ten were carried off by fits of apoplexy. Is it not probable that a fatal termination of this kind might be frequently prevented by a copious bleeding from the vessels of the feet?

Purgatives are of the utmost importance in the treatment of insanity. In periodical insanity the paroxysms are usually preceded by obstinate constipation. A dose of purgative medicine will, at this early period, frequently put a stop to the progress of the attack. This fact ought never to be lost sight of. A case of mania is related, which was apparently cured by suppuration of the right parotid. Another of melancholia, which disappeared in the same way, upon an attack of jaundice. Although it is impossible to know whether any individual case of insanity is curable, yet certain general rules may be drawn, as one kind of the disease is more frequently cured than another. According to the experience of M. Pinel, religious melancholia, mania complicated with idiotism, and regular periodical mania, are seldom or never cured.

We have now arrived at the conclusion of M. Pinel's book, from which we have extracted for the use of our readers those parts which promised either amusement or instruction.

The author has attempted a classification of the diseases of the mind, but this nosological attempt we have entirely omitted, because we thought it would be only troubling the reader to little purpose. The translator has annexed an introductory account of the old opinions upon the subject, in which he displays a respectable portion of reading.

We do not think the book is of much value, as we believe it would be difficult to find in it one single idea, possessing the two recommendable qualities of novelty and importance. Any one who is familiar with our English treatises upon this subject, will receive but a slight accession to his knowledge by reading the treatise of M. Pinel; and if we acknowledge, as in justice we must, that the volume before us contains as much information on the subject of insanity as the volume which we previously possessed, we must declare at the same time that this acknowledgment is rather a satire upon our old books than a compliment to the new one.

In times when large books are so plentiful as at present, 288 pages is nothing so very immoderate, and yet, although we have read the book more than once, we find it difficult to explain how M. Pinel could manage to spread so little matter over so large a surface. The book is copiously sprinkled with short chapters, in which the author proses a great deal, but in which we can find nothing but French exclamations and French grimace. These should have been omitted in the translation. Almost all books of science, upon being transferred from one language to another, would bear the same paring away. Let the introductions, prefaces, general observations, and cramming matter be thrown aside, and let us have nothing but the facts and the inference.

The wise merchant imports foreign goods in the compactest form, in order to lessen the cost of importation.

ART. V. *An Essay on the Nature of Fever, being an Attempt to ascertain the Principles of its Treatment.* By A. Philips Wilson, M. D. F. R. S. 8vo. pp. 210. London, Murray. 1807.

THE times in which we exist are truly eventful ones; we have lived to see

> " Atoms and *systems* into ruin hurl'd,
> And now a *bubble* burst, and now a world. "

Political and ecclesiastical institutions, which it had been the work of ages to establish, and had become venerable by their duration, are now overturned by a *fiat*; and new codes of law and of religion are imposed with a boldness and success unexampled at any former period. The world of science too has undergone its revolutions, and medicine has partaken of these in no inconsiderable degree. The work before us is a fresh attempt at something of this kind.

It is no doubt within the recollection of most of our readers, that the author of the present essay is not a mere *tyro* in the labours of the pen; he having some few years ago presented the public with four large octavo volumes upon the subject of Febrile Diseases, the materials of which had been delivered in a course of lectures at Edinburgh in the year 1796; and professing to attempt an object before unattained, viz. " to present, at one view,

iew, whatever in the present state of medicine it is requisite or the physician to know, respecting the symptoms, causes, and cure of those diseases!" In that work, which the author tells us was the result of "several years study," he affirms that "of the medical systems which have been proposed to the world, Brown's alone is in a great measure founded on observation. Many parts of it are simple deductions from facts which must be admitted independently of all hypothesis:" that "Dr. Brown alone has made several steps towards a true system of medicine";—and that—"in a more advanced state of science, when the systems of Stahl, Hoffman, Boerhaave, and Cullen are forgotten, there will still remain enough of the Brunonian doctrines to preserve the memory of their author." !!!—Yet something still was wanting; even this great luminary of medicine, whose light was to guide us *in wandering mazes lost,* even he, we are told, "in taking a few right steps, has (as might be expected from the state in which he found the subject) taken many wrong ones."—"To separate the true from the false" then, and—"to advance a few steps in addition to those which Dr. Brown had taken, towards a true system of medicine,"—was the important task which Dr. Wilson then proposed to himself. Alas! how short is human foresight! In the space of only four years, four little years from the time at which the *second edition* of that work was published, (1803)—that work in which it was so authoritatively predicted, that Brown alone would stand the shock of ages, we find *him* too laid in the tomb of all the Capulets! But let Dr. Wilson, who chaunts the requiem, speak for himself. "If the reader (says he) has seen the account of Dr. Brown's hypothesis in the 1st vol. of my treatise on fevers, he may probably remark, that its merits are there estimated more highly than in the work before him. The truth is, from its having been the favourite system among the students of the university of Edinburgh at the time I studied, I had conceived a strong prejudice in favour of it before I was capable of estimating its merits, and it was long before I could persuade myself, that it had in fact made no real addition to our knowledge." Note 4 in the Appendix p. 104.

Why this pointed recantation of error is thrust into the Appendix, instead of appearing where it ought to be, in the very beginning of the Essay, as the best reason for its being written at all, is a question of minor consequence. Let us rather consider the *force* than the *place* of the argument it contains, for adopting the Brunonian *hypothesis,* viz. "its having been the favourite system among the *students* at Edinburgh;"—yes, the favourite system among those who were only learning the rudiments of their profession, and who, like their great leader himself, had never witnessed a sufficient number of cases either to verify or disprove the doctrines they espoused! Yet, upon the credit of *such persons' belief*, did our author not only embrace the same principles, but taught them again to those, who, as being younger students than their instructor, must be supposed still less "capable of estimating their merits". How far Dr. W. was successful in his attempt upon that occasion, it is beyond our duty to go back and inquire; but for the sake of those who might have been his hearers then, or his leaders since, we hope the impression made was not indelible; for they are now called upon to abandon those principles which they had acquired, which rendered the practice hitherto "unsteady, complicated, and ill defined,"—and to adopt what are here offered, upon *the same authority*, as at once rendering the treatment of fever sufficiently obvious, and necessarily leading to that which experience has pointed out to be the best. (p, 138, 9.)

Dr. W. sets out with bespeaking the attention of his readers by the following introductory paragraph.

"From the silence of physicians on the Proximate Cause of Fever, since the appearance of Dr. Cullen's FIRST LINES, and Dr. Brown's ELEMENTA MEDICINÆ, they appear to have regarded its investigation as a hopeless labour; and it had, before this period, baffled the attempts of so many, that the following Essay would require some apology, were it not that in the advancing state of science, every new inquirer has better data than those who preceded him."

We are no advocates for the opinion, that the truth is absolutely unattainable with respect to any of the phenomena of nature, and least of all are we disposed to think so with regard to those which concern human health; as it would imply, that Providence had doomed man to irremediable suffering; while we daily see new expedients devised and new remedies successfully employed, for the alleviation and cure of disease. We confess, however, that on the point in question, our expectations are small; not merely because such men as STAHL, BOERHAAVE, HOFFMAN, CULLEN, and ———— *Brown*, have failed, but because, (contrary to the author's assertion) the new inquirer has *not* better data than they possessed: for what important discovery in anatomy, physiology, or pathology, has been made since the two last of these writers, that could afford any new and more satisfactory ground to build upon? The hope of better success, then, must, for the present at least, rest entirely upon a more dexterous management of the materials already accumulated; and how far the author has excelled his great predecessors in this task, we shall now proceed to examine.

Previous to the annunciation of his own theory, Dr. W. states some of the principal doctrines upon the subject, beginning with Hoffman and Boerhaave, and ending with Cullen and Brown. This method, if fairly pursued, is certainly allowable, not only as being sanctioned by great examples, but as being attended with some advantages; for as medical truths do not, like mathematical ones, admit of incontestible demonstration, we must rather approximate to them by circuitous, than arrive at them by direct steps; and we often gain more by shewing what is wrong, than by trying at once to prove what eventually may be right.

Hoffman's doctrine is briefly stated in a quotation from his own works, and it is dismissed by our author, with another quotation from Dr. Cullen, acknowledging our *obligation* to Hoffman; but not a word more is said upon either its merits or *defects*. Boerhaave's system notwithstanding its "consistency and simplicity" (p. 20) is passed over still more summarily; and the author proceeds to the "hypothesis" of Dr. Cullen. For the statement of this, however, as beyond our limits, we must refer our readers to the Essay, where it occupies eighteen pages; unless they should prefer seeing it in its original situation, in that illustrious professor's First Lines. Dr. W. then attacks the ground-work of Dr. Cullen's theory; and he makes his first approaches under a concession which the candour and modesty of that great man drew from him, not only respecting certain fundamental positions, but respecting *his own* claims; wherein, after allowing—"that although the Vis Medicatrix Naturæ must unavoidably be received as a fact, yet wherever it is admitted, it throws an obscurity upon our system;" (Pref. to the First Lines)—he afterwards concludes by observing, "this which I have attempted, may, like other systems, hereafter suffer a change; but I am confident that we are at present in a better train of investigation than physicians were in before the time of Dr. Hoffman." (ib.)

Upon this Dr. W. remarks—"But setting aside what Dr. Cullen says of his doctrine, if we examine the doctrine itself, we shall find that it is wholly constructed on a hypothetical basis, on the *supposed* operations of the Vis Medicatrix Naturæ, respecting which we have in the Introduction to this essay given Dr. Cullen's opinion." (See above.)—"How the debility of the nervous, proves an indirect stimulus to the sanguiferous system; how this stimulus acts in exciting the cold stage and spasm; how, through the intervention of these, the action of the heart and larger arteries is increased; is only explained by reference to the operation of the Vis Medicatrix, that is, is not explained at all; and yet it appears, even at the first view, that on these the whole system rests." (Ess. p. 40)—"Shall we suppose Dr. Cullen, after declaring that, 'whenever the Vis Medicatrix is admitted into medical systems, it throws an obscurity upon them'—so inconsistent as to offer as a true system, one wholly and avowedly founded on the supposed operations of this agent." (ib.)

We are no sticklers for the Cullenian doctrine of fever; on the contrary we consider it as defective in theory, and unsafe in application; but we are friends to fair criticism, and the upholders of truth as far as it enters into any system. It appears to us, then, that Dr. W. has jumped over the substantial fact, and eagerly caught at the' shadowy concession stated by Dr. Cullen; or rather, he has unfairly made use of the "obscurity" acknowledged to attend the admission of a Vis Medicatrix, by way of argument against the *existence* of the thing itself, which, Dr. Cullen says, "must unavoidably be received as a fact." Nay, with singular inconsistency, the existence of such a power is admitted in the Appendix to the essay, and examples given of its operation in the vomiting and purging excited to expel indigestible substances, in the inflammation, suppuration, and discharge of matter, occasioned by a thorn thrust into the skin, which last, (it is added), continues until the whole of the offending substance is washed out, (p. 160.) But this is not enough for Dr. W. for he asserts that "it is of little use to know that poison is expelled by an effort of nature, if we are not at the same time informed of the steps by which nature expels it." (Ib. p. 162.) Indeed! Is it of no use that nature has thus pointed out to us an operation by which the body is preserved from destruction, and by inducing which *artificially*, we can ensure the same benefit, without risking that mischief which may take place before she is roused to exert herself? It is as unworthy the character of a man of science to argue against a general principle, like the V. M. N. because absurdly reasoned upon by V. Helmont, Stahl, and Nichols, as it would be in a philosopher to deny the attraction of matter, because he could not perceive the palpable chain which drew distant bodies together.

What should we say to the discretion of the mariner, who rejected the aid of the compass, because he could not explain why the needle turned one end towards the north, and the other towards the south pole; or because he observed that it did not at all times and in all places exactly maintain the same relative position? And what must we think of the judgment of the physician, who discards the idea of a Vis Medicatrix Naturæ, because

cause he is unable to trace the several steps by which it operates; or to comprehend the reason of its occasional anomalies?

There are many ultimate facts with the bare knowledge of which we must on several occasions rest satisfied; and which, when ascertained to exist, can be usefully applied to the explanation of various phenomena in theory, and to the most beneficial purposes in practice. Such, it appears to us, is the Vis Medicatrix Naturæ; and whatever may be Dr. W.'s sentiments respecting it *now*, we will venture to predict, that a little more *experience* will make as great a change in his opinion on this point, as it has already done upon many others.

Dr. Cullen's error, then, does not consist, as Dr. W. alleges, in admitting a Vis Medicatrix, for in that he is justified by the concurring testimony of the ablest physicians in every age, and borne out by daily observation; but it consists in believing that it is the cause, not merely of the *re-action* of the system, as shewn in the symptoms which arise during the *hot stage* of fever, but that it also occasions a part of the *cold fit and spasm* which precede the hot stage. The idea which we universally attach to a preserving and healing power, is, that of counteracting the effects produced by the morbific agents in the first instance, which it does by exciting a new and *opposite* set of symptoms. We can readily see therefore, why the coldness, general debility, and feeble action of the heart and arteries, which, are the direct effects of the sedative causes of fever, should call forth the Vis Medicatrix, and thereby excite increased heat, &c. by which these hurtful effects are removed. A still weaker point of Dr. C.'s theory is, his assuming *spasm* of the extreme vessels, as the essential or proximate cause of fever; seeing that it is only *one* in the train of symptoms which constitute fever, and not the *primary one;* for he himself tells us, that it is preceded and caused by *atony*; but farther, because fever often occurs, not only without sensible spasm of the extreme vessels, but where an opposite state of relaxation, shewn in profuse and long continued sweating, is very conspicuous.

The objections which have been often brought against the Cullenian system, and the whole force of which is comprised in what we have said in the preceding paragraph, are repeated by Dr. W. but certainly without gaining any additional strength in his hands, for he still lays the chief stress upon the non-existence of a Vis Medicatrix, as will be seen in his conclusion:

"Dr. Cullen's hypothesis then appears to be doubly objectionable, as resting on the supposed operations of the Vis Medicatrix Naturæ, and being inconsistent with observation and the known laws of the animal economy."—"As an hypothesis (he continues) to serve the purpose of connecting together a long catalogue of facts, till we should arrive at a more explicit knowledge of the nature of fever it is elegant and ingenious; and admitting the data, accounts for all the phenomena of the disease; but Dr. Cullen is every where ready to confess the insufficiency of these data."

No doubt the author can reconcile the seeming contradiction of calling that "elegant and ingenious," which we are told is "inconsistent with observation, and the known laws of the animal economy."

He next turns his attention to a still more modern writer, once (as we have already seen) his *magnus Apollo*, of whom he speaks thus:

"Dr. Brown, on the other hand, with data no less, though apparently not so ill founded, has endeavoured to conceal the defects of his hypothesis, and give it the appearance of what he wished it to be thought, a true system of medicine; and it is so blended with certain positions, of the accuracy of which there can be no doubt, and which were universally admitted before Dr. Brown wrote, that to detect its errors it will be necessary to examine it in detail." P. 46.

Those who might be gratified by such a long detail of *errors*, we refer to the essay itself, where perhaps it may be thought to have more weight, as coming from a quondam admirer; but if they wish to see their fallacy *clearly* displayed and *radically* controverted, we would recommend the III. number of the Edin. Medical Journal, where the subject is treated in a masterly way, upon the occasion of a new and complete edition of Dr. Brown's Works being offered to the public by his son. We would merely observe, that Dr. W. in addition to his former acknowledgment of incapacity on the score of judgment and experience, to estimate the merits of Brown's system, has here relinquished any claim he might have been allowed as a man of reading, and of information in the history of his profession; by thus informing us, for the first time, that the only merit it possesses consists in its defects being, "with a view to concealment, blended with certain positions which were universally admitted before Dr. Brown wrote." If such were the fruits of "several years' study," what are we to expect from only "four years of experience," upon which all the "new and obvious principles" of the present essay must be grounded? (P. 58, of this journal)

Having thus dispatched four doctrines of fever as *baseless fabrics*, our author proceeds to state his own ideas, or those which *he* considers as exclusively his, respecting the proximate cause, symptoms, and cure of fever. These, which are comprehended in the second chapter, we shall state in as connected a way as we can; but must occasionally break the continuity of the narrative, by remarks, lest we should overlook passages which are contradictory to facts or to one another.

He begins by censuring as improper, the universally received division of functions, into *vital, natural,* and *animal,* alleging that while the two first differ widely from the last, they so run into one another, as to render it impossible to draw any precise line between them.

"The objection is strengthened (he adds), when we consider the offices of the different sets of functions. By the animal functions the individual is connected with the external world; by the vital and natural, his own existence is preserved."

"We see again the impropriety of the division, if we turn our attention to the sources from which the organs of these functions immediately derive their power. The animal functions depend immediately on the brain and its appendages. The vital and natural functions, though influenced by the state of the nervous, depend immediately on the sanguiferous system, and are perfectly performed where the brain has never existed, as in the case of a fœtus born without a head."

"It may also be observed, that of the vital and natural functions the animal is unconscious; of the animal functions he is not only conscious, but many of them are subjected to his will." P. 89, 90.

We have seldom seen more palpable contradictions to fact, than are contained in these two short paragraphs: "the animal functions depend immediately on the brain and its appendages;" admitted;—but the vital and natural functions, though "influenced by the state of the nervous, depend immediately on the sanguiferous system, and are as perfectly performed where the brain has never existed, as in the case of a fœtus born without a head." According to the division of functions which *we* were, (it seems erroneously) taught, that of the brain and its appendages held the very *first* rank in the *vital* class; being, indeed, that which, as inherent in the nervous substance, remains for a short space of time after all the others are destroyed, and that without which not one of the others can, even for a moment, be performed. We have both heard of and seen acephalous fœtuses, but we have never either seen or heard of one, in which not only the *brain,* but its *appendages* also, (as the nerves undoubtedly are), were wanting; for without nerves, how are the heart and arteries to be sensible to the stimulus of the blood? As little have we ever heard of even the simply brainless monsters performing the function of respiration, except in the *mendacious* accounts of Baron Munchausen.

"Of the vital and natural functions the animal (man?) is unconscious." Respiration is one of the vital functions; Is man then any more unconscious of this, than he is occasionally of his situation as to motion or rest? How often do we forget that we are walking, until we run against something?

"The organs of the vital and natural functions, on which life depends, possess an excitability which never suffers any diminution, except in disease; nor complete exhaustion, except in death. Those of the animal functions which suffer intermissions that they may relieve the vital organs on which their action depends, possess an excitability which suffers a constant alternation of exhaustion and renewal; for it is only for a certain length of time, that the vital can, without interruption, support the action of the animal organs." P. 90, 1.

If the excitability of the *vital* organs never suffers any diminution in health, why is it that the *animal* functions "suffer intermissions, that they may relieve the vital organs on which their action depends?" The author has here gratuitously supplied us with an answer completely subversive of his own false position, viz. because "it is only for a certain length of time that the vital can, without intermission, support the action of the animal organs;" in other words, the vital and natural organs do *not* possess a uniform and undiminished excitability in health, but, in common with the *animal* organs, require an interval of lessened action to recruit themselves, and again supply these last.

Dr. W. influenced by the *weighty* arguments just mentioned, rejects the ordinary division, and classes the two first sets under the indiscriminating title of *vital functions*; and how greatly this contributes to a clearer comprehension of the subject, will presently appear.

"*It* is evident, therefore, (he says), that when the vital organs are debilitated, the animal organs necessarily partake of the debility. It is equally evident why the *converse* of this is not true; the nervous system may be debilitated without affecting the powers of circulation. Thus, when any cause impedes the action of the heart, the brain immediately partakes of the disorder; but even in apoplexy, the pulse in general remains strong and good, and only becomes slower and more languid, in proportion as the respiration, which is performed by muscles that depend for their vigour on the influence of the brain, becomes so."

"Thus it is, that in diseases properly termed nervous, the pulse is generally good, however great the languor and depression of strength may be; nay, I have even found, from experiments made for the purpose of ascertaining this point, that the total destruction of the nervous system produces no immediate effect on the action of the heart, nor in any other way affects its motion, than necessarily happens in consequence of the interruption of respiration.

"The heart is possessed of little sensibility, so little, indeed, that abscesses and ulcers are often formed in it without giving any pain; and is, considering the size and importance of the organ, scantily supplied with nerves, the use of which seems to be to convey from the sensorium to the heart certain impressions, not to give vigour to an organ which we find in a state of perfect vigour, where no sensorium has existed." p. 93-4-5.

To our dull apprehensions, the converse of the position stated in the first of these three paragraphs, would be, that the *animal* organs may be debilitated, without affecting the *vital* organs; but instead of this, we are told that the *nervous system* may be debilitated without affecting the powers of circulation, in other words, that the *nervous system* may be debilitated, while its own *powers* remain unaffected; — for what are the *powers* of the *circulation* but those which are derived from the *nerves* of the heart and arteries, which render them sensible to the stimulus of the blood? The author, in proof of his position, says, that in apoplexy, the pulse is affected only through the medium of the respiratory organs, which depend on the influence of the brain;—and finally, that the *total destruction of the nervous system* affects the heart's action in no other way;—Here is literally multum in parvo, few words and many absurdities; for it would be a gross dereliction of our duty to rank them among the venial errors, into which unpractised thinkers and writers occasionally fall. That the heart must soon cease to act after the supply of blood to it is cut off, by a stop being put to respiration, is so obvious in itself, and so uniformly proved by observation as to admit of no dispute; and that an irregular and imperfect breathing will often considerably affect the pulse, we readily admit. But that the slower and more languid circulation in apoplexy is caused only through the medium of the respiratory derangement, is a mere petitio principii, neither probable in itself, nor agreeing with what we know respecting the source and series of symptoms in this disease. Does not the heart as well as the lungs, perform its office by the nervous influence derived from the brain? And are not both paralysed by cutting off the communication with this source of nervous energy? Why, then, should not the heart as well as the lungs be affected immediately by the morbid state of the brain? Because (the author says) "the total destruction of the nervous system produces no immediate effect on the action of the heart, as he has found by experiment;—yes, by experiment on *frogs*, the economy of which is so different from that of man and the more perfect animals, that they will move about for some time after the heart is cut out or the brain destroyed.

But is this the case in man* ? and if not, where is the use of such experiments to illustrate fever in him ?

A singular fatality attends Dr. W.'s illustrations, viz. that they often serve to confute his positions: so, in the present instance, we are told (see last par. quoted) that the use of the cordiac nerves seems to be, "to convey from the sensorium to the heart certain impressions, not to give vigour to an organ which we find in a state of perfect vigour, where no sensorium has existed." Whether purposely or not, the expression is very obscure, and two irrelevant facts are arbitrarily coupled together, by way of elucidation. The " certain impressions " alluded to are no doubt, the passions, the immediate and powerful influence of which on the action of the heart, is well known; do these too affect the pulse only through the respiration ? No, but by directly increasing, as in anger, or diminishing, as in fear, the supply of nervous energy from its great fountain the brain, the heart has its action *immediately* rendered correspondently strong or weak. The heart being vigorous in a brainless fœtus, is of no weight to the author's position; as the brain only commences its vital influence, when the young animal assumes its independent existence; and the same is the case with every important organ, the heart and blood vessels excepted, for, in the fœtal state, the stomach, intestines, lungs, and liver, are not yet requisite to life. But to proceed†.

(A) " It appears from what has been said in the preceding part of this essay that the terms excitement and exhaustion, in the sense in which Dr. Brown uses these terms, apply only to the animal functions, and are merely expressions for the healthy states of vigilance and sleep. There is no debility which can be induced on the animal organs, while the vital retain their vigour, which is to be regarded as morbid, because, under such circumstances the vigour of the former is always restored by sleep.

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* We were here about to exclaim with Macbeth :

The times have been,
That when the brains were out, the man would die,
And there an end : but now, they rise again
With twenty mortal murthers on their crowns,
And push us from our stools : this is more strange
Than such a murther is !

but from this we were recalled by the simple question, *is it so in man?*

† To prevent unnecessary repetitions, we have affixed capital letters to each paragraph, and small letters between brackets, to the individual passages which we shall have occasion to refer to.

(B) " But

(B) " But if the excitement of the animal functions be continued till it occasions debility which support it, a state very different from exhaustion is the consequence. The steps by which exhaustion is changed into this state, which may properly be termed atony, are easily traced.

(C) " A certain excitement of the animal functions continued for a certain length of time, occasions a tendency to sleep. This tendency increases as the excitement is protracted, as long as the sanguiferous system properly performs its functions. But if the excitement is continued beyond this point, a degree of restlessness comes on. Instead of a healthy action of the heart and arteries, their pulsations gradually become more feeble and frequent, and the various functions, all of which depend more or less directly upon the sanguiferous system, shew a corresponding debility.

(D) " The tendency to sleep at length ceases, sleep being no longer capable of restoring vigour. It is in vain to abstract light from the eyes, and give rest to the limbs, the powers which should renew the excitability of the animal organs, are themselves in want of refreshment.

(E) " The only means of restoring these powers to a due action, is applying to them a stimulus stronger than the natural stimulus, in proportion as their excitability is less than it ought to be; for the system, it is evident, possesses no organs, by which the vigour of those of assimilation, that is of the vital organs, can be restored.

(F) " The application of a preternatural stimulus to them, is the unavoidable consequence of their debility; for the secreting organs no longer performing their functions, the more irritating and noxious parts of the blood, which ought to be expelled from the body, are retained, and soon excite the heart and blood vessels to an action as powerful as the healthy action.

(G) " It is to be recollected, that all debilitating causes applied to the vital organs, are felt most in those parts which are least vigorous, that is, at the greatest distance from the heart.

(H) " These, to avoid circumlocution, I shall call the circumference of the system; the heart and larger vessels I shall call the centre. Of the former are composed the different secreting organs. Hence debility of the vital system is first indicated by a failure of the various secretions.

(I) " If the increased action of the heart and larger vessels, as frequently happens, by applying an increased stimulus, restores vigour to the circumference of the vital system, the action of the secreting organs will be renewed, the offending matter expelled, and health restored. If not, the preternatural excitement of the heart and larger vessels must go on till the debility extends to them, and there being no longer any means of excitement in the system itself, if the debility induced be considerable, the animal dies. Death may frequently be prevented by exciting the heart and arteries by artificial means, until they have restored power to the capillaries.

(K) "I have been considering the effects of too violent or long continued excitement of the animal organs. It is evident, however, that similar effects may be produced in two other ways. If we suppose a noxious power received into the sanguiferous system, capable of debilitating the heart and blood-vessels, whether previous excitement has taken place or not, the same want of vigour in the excretories, and increased action of the heart and blood-vessels, will ensue. These symptoms, and their consequences, may also take place without any debilitating power applied to the heart and blood-vessels; for these remaining in their natural state, if any cause impede the proper action of the excretories, the same increased action of the sanguiferous system will take place, but it will not be preceded by equal marks of debility, but only by those which attend the failure of the excretions.

(L) "It appears then, that when a debilitating cause is applied to the vital system, the extreme parts of this system lose their tone; that in consequence of this, secretion being impeded, a preternatural stimulus is applied to the heart and larger vessels, which by exciting them, tends to restore tone to the capillaries, in the same way that an increased action of the vessels of an inflamed part, tends to restore tone to the capillaries of that part. On this principle, I believe, the whole phenomena of fever may be explained; to establish this position, it will be necessary to review these phenomena, as they occur under the three heads of the symptoms, causes and cure of fever."

As this part of the essay certainly contains the essential points of the author's system, we have here given a very long and continuous extract from it, not only that our readers might be enabled thereby to form their own judgment, but that *we* might not be considered as taking any unfair advantage, by offering them in a detached and mutilated state: to make up for the room which this necessarily occupies, we shall consult brevity in our comments, by merely referring to letters marking the particular passages criticised.

Par. A.—The conclusion drawn here has already been disproved by us; and though no friends to the Brunonian system, we think this position of it is universal,—that *every* organ and function requires an interval of comparative rest to recruit its powers.

Par. B. and C. not only afford an additional proof of this, if any were wanting, but go a step farther, and inform us (which the author has failed to do) *why* the pulse becomes frequent and feeble, viz. that the nervous system being no longer able to supply a degree of influence adequate to the demand; so as to keep up a steady action; now by means of the Vis Medicatrix (*pace auctoris*) makes efforts which are frequent and feeble in proportion as there is (in the figurative but not unapt language of Mr. J. Hunter,) a consciousness of deficient *power*. Whether this state, then, is "very different from exhaustion," we leave the impartial reader to decide

Par. D.—The reason given *why* sleep ceases, viz. its being no longer capable of restoring vigour, is neither obvious in itself, nor is it consonant to facts either direct or analogous. Does sleep cease to recur, when a person rises as heavy and unrefreshed as when he lay down? If so, we have known persons who did not sleep for months together. Does the appetite cease when the food no longer restores strength and flesh? On the contrary, in Tabes Mesenterica, we find the appetite ravenous, while the body is daily wasting from defect of nourishment.

Par. E. affords a proof of the confusion that arises from the worse than useless change of terms adopted by the author, by the organs of assimilation being classed among the *vital* ones, or those immediately necessary to life. In the ordinary and (in opposition to Dr. W.'s authority, we will still say) *proper* division of functions, we see a beautiful order and arrangement; that while the exercise of the animal functions are requisite to the healthy operation of the natural ones, these last are, in their turn, necessary to maintain the structure of the *whole* machine, and consequently of the vital organs, the functions of which again, by a reflected influence, give *power* to every part, so as to maintain the existence of the system.

Par. F. and I.—After what was stated in the Introduction, p. 5, "that fever is not owing to any change induced on the fluids, their becoming *too acrimonious*, too viscid," &c.—and the farther reprobation of the humoral pathology in p. 11-13 of the essay, we were little prepared to expect either as a new discovery, or as consistent with the author's professions, this self same humoral pathology brought forward to explain the phenomena of fever, in the identical way in which it was employed by Hippocrates and Galen, has been so much dwelt upon by the older writers, and is still daily recurred to in the phrase of *suppressed perspiration*. We by no means assert the entire falsity of the opinion; for we believe the general idea is just, and even that it can be proved so, though not exactly in the mode Dr. W. has done; but we hold it at once uncandid and dishonest, to abuse the medical worthies of former days, for opinions of

which

which we strip them merely to deck ourselves, without acknowledgment.

Par. G.—Spirituous liquors drank to intoxication, will certainly be admitted as *debilitating causes;* these are applied to the stomach, which our author considers among the *vital* organs; but do we not often find persons stammer, and shew confusion of ideas, while they can not only walk steadily, but make exertions in running, &c. far beyond their powers when sober?

Par. L.—When the reader has attentively perused this, let him turn to the doctrine of fever as summed up by Dr. Cullen.

"Upon the whole, our doctrine of fever is explicitly this: The remote causes are certain sedative powers applied to the nervous system, which diminishing the energy of the brain, thereby produces a debility in the whole of the functions, and particularly in the action of the extreme *(and consequently secreting)* vessels. Such, however, is at the same time the nature of the animal economy, that this debility proves an indirect stimulus to the sanguiferous system, whence by the intervention of the cold stage and spasm, *(or constricted, and of course non-secreting state)* connected with it, the action of the heart and larger arteries is increased, and continues so, till it has had the effect of restoring the energy of the brain *(vital organs)*, of extending this energy to the extreme *(secreting)* vessels, of restoring, therefore, their *(secretory)* action, and especially overcoming the spasm *(constricted and non secreting state)* affecting them; upon the removal of which, the excretion of sweat, and other marks of the relaxation of excretories take place."

Where, then, is the difference that justifies Dr. W. in pronouncing this an *unfounded hypothesis*, and his own, forsooth, a *demonstrable and consistent theory;* is it in Dr. Cullen's alleging that a considerable portion of blood which in health circulates freely through the extreme vessels, is, in the cold stage of fever, owing to the loss of tone in these vessels, accumulated in the heart and larger arteries; where, by a law of the system ultimately calculated for its preservation, and therefore justly termed a Vis Medicatrix Naturæ, it rouses by its increased quantity, a re-action of these, so as to give rise to the symptoms of the hot stage, and the complete or partial solution of the disorder? or is it in our author *assuming* that it is not by the accumulated *quantity*, but the increased *acrimony* of the blood that this is done; and that, although the result is to preserve life, yet that a preserving power has no concern in it? The answer to the first is simple; the accumulated *quantity* is obvious and universally admitted; while the increased *acrimony*, though it may be true, yet remains to be proved; and is if augmented stimulus from quantity will explain the matter, according to the laws of philosophizing, unnecessary. The answer to the second part of the question, is furnished, (as has been the case on several other occasions) by the author himself; for, only three pages farther on, after retailing the opposite symptoms marking the *hot* as succeeding to the cold stage of fever, he concludes, "this *general effort of the system*, restores the vigour of the secreting organs, and all that should have been gradually separated from the blood, is now forced off at once by the alimentary canal, kidneys, and skin." Though the virtual admission of a preserving power, is here clogged with an explanation of the author's, it is not invalidated by it; for whether the stimulus be from *quantity* or *quality*, can make no difference.

Having been so full in our examination of what is certainly the essential part of the author's doctrine, and what he alleges to be *novel*, viz. the *hypothetical* part, we think it unnecessary to consider particularly the account he gives of the symptoms, causes, and cure of fever; seeing that the two former are entirely copied from preceding writers, especially from Dr. Cullen; and the last, whatever merit it may have, does not stand in need of Dr. W.'s authority to recommend it, being, (as he himself tells us,) that which experience has already pointed out as the best. (p. 138). There are indeed numerous passages under each of these heads, which would admit of criticism, had we either room or time for it; but *ubi magna fœdant chartulam, haud parvis offendar maculis;* we shall therefore only notice a few, to prove that the author has not shewn much more accuracy or consistency here than in his hypothetical lucubrations.

Symptoms of fever, p. 122. "When the contents of the stomach and alimentary canal stagnate, they are as apt to become putrid as the same matter out of the body exposed to the same degree of heat and moisture, unless the antiseptic fluids are supplied to check this tendency. They therefore become putrid in fevers of long continuance, and the more readily if there be present a considerable portion of bile which is a septic.

"A similar change takes place on the surface of the body. The failure of the due secretion and absorption there, causes a stagnation and putrefaction of the natural moisture. Hence the putrid smell of the sweat."

Here we have as thick a tissue of errors as could well be woven into so small a compass. We deny that the contents of the stomach ever become actually putrid during life, unless they were so when swallowed; and challenge Dr. W. to cite, on any other authority than his own assertion, a single example of *putrid* matter being evacuated by vomiting in fever. Even the intestinal contents, though highly fetid, never have that *kind* of fetor which putrid flesh emits; and they are often more offensive when scarcely any food has been taken, in short, when they consist entirely of the diseased secretion poured into the intestines. What the *antiseptic fluids* then are, which check this tendency, the author has not vouchsafed to inform us; and indeed will find it difficult, as we know of no fluids produced here, but those very ones upon a morbid state of which this *putrescent* condition of the stools depends. With respect to the *septic* quality which he attributes to the *bile*, we thought this error of Boerhaave had long been corrected by common observation, that the parts tinged with bile by transudation after death, were longer in becoming putrid than the neighbouring ones; and whatever more was wanting, is fully supplied by the direct experiments of Dr. Saunders [Treatise on the Liver, p. 135-6,] and others, which prove that bile putrifies much more slowly than blood. We can likewise tell the author, what his *experience* has not yet, it seems, been sufficient to teach him, that in proportion as bile is brought into the intestines in fever, in that proportion does the fetor of the stools lessen, insomuch that when the evacuations consist of little else than bile, they have scarcely any odour but the faint smell proper to this fluid. The reason too is obvious; the intestinal canal being now supplied with its natural stimulus and tonic, the fluids secreted from its surface are of a more healthy kind, and their putrescent tendency is still further corrected by the *antiseptic* quality of the bile.

P. 137. Speaking of contagion as one of the remote causes of fever, he says, "Now in the inoculated small pox, we are assured that the first impression of the contagion is on the heart and large vessels, for the fever does not commence till matter is formed in the inoculated part, and received into the sanguiferous system by means of the absorbents; and it would seem that in typhus and the natural small pox, a certain time is required, as in the inoculated small pox, before the contagion to whatever part applied, occasions the secretion of such a matter as, when absosbed, shall produce the disease."

That the variolous fever from inoculation is *commonly* preceded by signs of absorption, viz. stiffness and pain of the axillary glands, is a fact; but that this is *not universal* is equally true; and, that the absorption of the matter first formed is indispensably necessary to produce the general disease, may be doubted from what we see in hydrophobia occurring in man from the bite of a mad dog. The poison here is infused into a wound which always heals up before the disease appears. When it becomes active, pain is commonly felt in the cicatrix, which often inflames; the irritation, however, does not follow the course of the *absorbents*, but that of the *nerves*, and where the bite is inflicted on the hand, this distinction can be clearly shewn. If this disease then, is communicated from the part to the constitution by means of *nervous irritation only*, what should prevent the small pox from being so? That the variolous matter should irritate the *absorbents* as well as the *nerves* leading from the part, is no objection; *any* acrid matter, of the purulent kind especially, will do so. Besides, if not demonstrable, it is in the highest degree probable, that in man there is no multiplication of the hydrophobic virus; for out of the multitude of cases on record, there is not a single attested one, of a hydrophobic patient communicating the disease to others, though they have frequently in their struggles, bitten those who held them. Lastly, we would ask, where is the proof, that either in natural small pox, or in typhus fever, the contagion is multiplied *before* the febrile symptoms commence? As to typhus contagion, we have only indirect evidence of its multiplication at all, viz. persons affecting each other in succession: but in casual or natural small pox, where from the mode of communication, we must presume the contagion to be inspired with the air, do we find pustules take place first, on the mucous membranes of the nose or fauces, to which certainly it must be applied first, and in greatest quantity? That these parts are highly susceptible of variolous inflammation, is often distressingly, and sometimes fatally experienced by patients; but they are affected only in common with other *atmospheric*[*] surfaces, and generally not till the *skin* is thickly beset with pustules.

P. 146. "Analogous to the office of the skin, the external surface of the animal system, is the office of its internal surface, if I may use the expression, the alimentary canal; and such is the sympathy between these surfaces, that if one is languid, the other is affected in the same way, and if we excite either, we, in a greater or less degree, increase the action of the other. If the bowels are constipated, we find the skin dry and *shrunk;* as soon as the bowels are restored to action, the skin becomes soft and moist, and vice versa, except when, the secretion by the skin being suddenly stopped, the fluid which should have passed by it, is thrown on the bowels, applying to their vessels a stimulus which prevents their inactivity."

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[*] As pustules have never been found on any but those surfaces to which the air has free access, we have employed the term *atmospheric* to distinguish them.

The sympathy between the skin and intestines is universally acknowledged; but so far from their being "*affected in the same way*," the very reverse is *generally* the case, as being in some measure vicarious outlets. Sweating and purging regularly alternate in the advanced stage of phthisis; and in the treatment of many disorders, this reciprocal alternation constitutes an important and successful indication of cure. If diarrhœa, succeeding to suppressed perspiration, depended on the cutaneous excreta being *thrown* on the bowels, why does it not equally happen, when slowly, as when suddenly done? The offending matter is equally retained in both cases.

P. 146-7. "We might therefore, *a priori*, have expected considerable advantage in fever, from exciting the bowels, not to mention, that their vessels form so large a portion of the circumference of the vital system. Every *physician must have observed* the excellent effects of supporting the due action of the bowels in fever, particularly in its early stages; but no writer has placed this in so clear a point of view as Dr. James Hamilton of Edinburgh. I have found the most decided advantage from exciting catharsis to the extent he recommends."

P. 151. "Physicians seem insensibly to be substituting a free discharge from the bowels, for venesection at this *(early)* period of the fever; and this change has not arisen from any reasoning on the subject, but from *experience* having taught them, that it is the most successful practice. The nature of fever itself, however, might have pointed it out."

Again, in the App. p. 203, after an extract from Dr. Hamilton's book on Purgative Remedies, in which, as will be seen by a reference to the original, that gentleman considers himself as the first who proposed the use of cathartics to any extent in the general treatment of fevers. Dr. W. subjoins,

"I might from *my own experience*, were it necessary, add many cases and observations in confirmation of these remarks. I shall only add, that I have very generally seen good effects from spontaneous diarrhœa, even in the worst forms of typhus; an observation which obtruded itself upon me *many years ago*, when with most other physicians, I was prejudiced against the use of cathartics in this fever, except as far as it was necessary for the regular expulsion of the fœces."

Well and truly hath the author said in the first extract given above, "*Every* PHYSICIAN *must have observed*," &c. but in saying so, he has made a distinction more just and consequential than he was aware of, between a young *graduate* who, (as Shakespeare's courtier talked of battles) can prettily discourse of maladies; and the *real physician*, who, having often faced disease, knows that *non eloquentia sed remediis morbos curari*. *(Cels.)*

(*Cels.*) If, however, the *high priori road* was so obvious; if "the nature of fever itself might have pointed it out," how came it to be overlooked by the author, not only in 1796, when he was a *graduate* and a *teacher*, but in 1803, when he had added seven years *opportunity* of practice to the several years during which he had devoted the whole of his time to the study of fevers? *(Pref. to the Treat. on Feb. Dis. v.* 1. *2nd edit.* 1803.) Dr. Cullen had said in his First Lines, that "the practice of sweating (in the general cure of fever) would be rendered safer by *moderate purging* excited at the same time;" upon which Dr. W. remarks: "The propriety of this seems very doubtful. It appears from a variety of observations, that the only purpose for which we ought to move the belly while we are endeavouring to promote sweat, is the regular expulsion of the foeces; and glysters in these cases are preferable to cathartics, because they are less apt to check the perspiration." (*ib. p.* 581.) And in the same vol. p. 662, he adds, "It is only in the earlier stages of the disease, when the contents of the alimentary canal may be very considerable, that cathartics of *any kind* are well suited to typhus."

Dr. Hamilton tell us in his Observations, that he first began his purging plan in fevers in 1778, and had continued it ever since. As he has been more than 25 years, to our knowledge, physician to the Edinburgh Infirmary, and as it is required that every candidate for a medical degree at that university, should enter for one year at least to the practice of the infirmary as conducted both by the clinical and ordinary physicians, how came it that Dr. W. was, even in 1803, unacquainted either with the nature or extent of Dr. H.'s purging plan in fevers? The dilemma is complete in whatever way it is taken; and we are constrained to say that Dr. W. was a *teacher* before he was a Physician; that, like the rest of the Brunonian school, he described diseases which he had never seen, and praised or censured practice which he had never witnessed; in short, that his *Lectures* in the first instance, and his four thick octavo volumes in the second, were, (as the latter intrinsically shew) a *cento* of quotations from other writers, worked up to a system in his chamber, with greater labour, but with infinitely less talent than we know the Elementa Medicinæ of his great prototype Brown to have been.

Possibly our readers may think that our criticism is too severe, and that it was unnecessary thus to "break a fly upon the wheel." But while we strictly adhere to the words of our poet, *(Speak of them as they are, &c.)* we have been somewhat roused on this occasion; first, by the manner in which the author treated the doctrine of our venerated and illustrious master, Dr. Cullen; and, secondly, by our observing in the perusal of the Essay, a most surprising coincidence between very many parts of it, and of the same subject, as contained in the printed *Outlines of a Course of Lectures on the Practice of Medicine*, as delivered at one of the principal medical schools in this metropolis; a coincidence so great, that the heads of the Outlines would nearly serve for marginal heads to the Essay. It is true, indeed, that the order of the Outlines is not strictly preserved in the Essay; in which, like the rays of light reflected from a shattered mirror, they often confuse rather than illumine the subject they are directed upon.

We are now to take our leave of the author, and for his sake as well as for our own, we hope it will be for a long time; sufficiently long indeed to give him that acquaintance with disease which will enable him to appear equally experienced as a practitioner as he has shewn himself industrious as a compiler. Should the *cacoethes scribendi* again return, and he feel inclined a *third* time to exalt himself into the chair of instruction, let him remember the gold enprecept of Horace, "*Nonumque prematur in annum*;" and when, instead of *four* short years, he has fulfilled the prescribed term, or even doubled it, he may perhaps abandon his juvenile projects, be convinced that our predecessors were not destitute either of talent or observation, and that he must be no common person who can correct the errors and supply the defects of such men as HOFFMAN and CULLEN.

ART. VI. *Observations on the Application of Lunar Caustic to Stricture in the Urethra and Œsophagus.* By M. W. Andrews, M. D. &c. &c. 8vo. pp. 174. London, Callow.

DR. ANDREWS is a candid narrator of facts evincing the efficacy of the caustic bougie. It is creditable to an author who engages in controversy, to abstain from party pretensions and personal animosity, because it affords substantial proof that he desires the public good. It is wise, because a judgment uninfluenced by private feelings prefers the strongest claim to public confidence. Our author modestly informs us in the outset "that he does not propose to offer any thing new, but merely to notice some of the principal objections that have been made to the practice, and to endeavour to produce additional proof in its favour." Of the anatomy of stricture in the urethra, our author says only as much as it was essential to premise to observations on the treatment. It is a subject which we think even the labours of Mr. Home have not perfectly elucidated. The common explanation proceeds upon the palpable petitio principii, that the membrane of the urethra being endowed with the functions of a muscle, possesses a muscular structure, though its fibres are insusceptible of demonstration. Yet in the common integument, and common hydatid, where muscular structure is not generally suspected to exist, the function of contraction is as evident upon the application of certain stimuli, as in the urethra. Cold contracts and warmth relaxes all these textures equally, and though none of them exhibits a trace of muscularity, one it is contended must possess it. Either then we must find stronger reasons for pressing the exception, or admit a very unphilosophical multiplication of causes. Again it may be said that stricture is spasmodic, and we may safely infer from analogy that the seat of spasm is muscular. But is not the canal of the urine closely embraced by muscle, and at that part which is peculiarly, if not exclusively the seat of spasm? But the formation of permanent stricture say the *muscle-men*, depends on the contraction of the transverse fibres frequently induced, and thus habitually narrowing the canal. Is not stricture of the most obdurate kind met with in the ductus nasalis? and will any Tyro be found so raw, as to assert that the lining of the antrum is muscular? But we are told that the membrane has the power of self adaptation in a very extraordinary manner to the emission of urine and of semen, that in the one operation it is enlarged, in the other straitened. Will any one who seriously compares the relative size of the penis in the opposite states of relaxation and erection, the steady uniform swelling of the accelerator urine with the convulsive grasp of the ejaculator seminis, be unable to explain the difference in the size of the streams, without supposing the membrane to possess an independent contractility. Let the urine be passed during the state of erection produced either by venery, cold, or the mechanical pressure of a distended bladder, and the diameter of the stream will not exceed that of the semen. If on the other hand the emissio seminis take place during an imperfect erection, when the preputium produces no constriction at the corona glandis, the converse of this fact will be equally notorious. Now if the membrane possessed the accommodating aptitude ascribed to it, it would dilate to admit the free passage of urine, notwithstanding the erection of the penis; and if it is said in this case to be denied capacity for dilatation, from the distention of the corpus spongiosum, &c, then it is fair to infer that the narrowing of the stream is owing to this cause, and not to its proper contraction.

But the result of this experiment Mr. Home would explain differently, for, says he, when the membrane is once contracted it remains so, until overcome by the action of its antagonist muscle the bladder, which forces it open in expelling its contents. This observation is glaringly incorrect if applied to the state of erection, the only state in which the healthy contraction of the membrane can exist. It overlooks in the erection from venery, the sympathetic relaxation of the erectores penis, in that from cold, the actual contact of the heated urine, in that from distention, the evacuation already made of the contents of the bladder.

These, and other changes must invariably precede so powerful a contraction of the muscle of the bladder as shall dilate the urethra to its natural size. In other words the erection must subside before a full stream of urine can be passed, and the diameter of the one will bear a proportion inversely to that of the other. In attempts to investigate the operations of nature in the animal economy, allowance enough is not made for the velocity of successive sympathies and motions; they are not traceable by our senses like the train of consequences in the inventions of human contrivance. But because we cannot embrace the whole extent, we are surely not to limit our view to a single agent, or to begin de novo to institute the catenation, from every point at which a link escapes us.

We might refer to anatomical facts in farther confirmation of our opinion, such as the intimate investment of the membrane of the urethra by the corpus spongiosum, the local connection existing between this and the cavernous bodies, the envelopment of all in a continuous ligamentous sheath, and the universal tension of the membranes, and even integuments, during the turgescence of the cells. But even allowing the muscularity of the membrane, the problem of the formation of permanent stricture recurs with undiminished difficulty. We can less readily suppose the starting of a muscular fibre from its level, than the puckering and projection of a fold of loose extensile membrane subject to frequent alterations in condition from the irregular action of the fibres that surround it.

A membrane that admits of such variable degrees of relaxation and tension as that of the urethra, in which the extreme states are induced at short intervals, must be liable to form plicæ and duplicatures. Such is the case with the peritoneum lining the abdominal muscles, the mucous membranes of the bladder, vagina, and rectum, the loose tunic of the testis, and similar textures covered equally by fleshy fibres. In combating, however, the alleged existence of a muscular sphincter urethræ, and the position that stricture is formed by its contraction, our object is rather to shew the insufficiency of the proof adduced, than the inaccuracy of the opinion they tend to establish. In all those cases, at least, in which the stricture does not describe a circle, but is confined to the under, upper, or lateral surfaces of the canal, and such are by no means rare, we must receive with caution, the statement; that "a stricture whether spasmodic or permanent, is a contraction of the fibres of the membrane which forms the canal." Indeed we must confess our total inability to comprehend the contraction of a circular fibre to take place *exclusively* in *one fifth part* of its circumference.

The investigation of morbid appearances in this disease, is attended unfortunately with more than ordinary difficulties; first because the disease is seldom allowed to prove fatal, and second because the spasm with which permanent stricture is more or less combined, vanishes with life. The simplest form of the disease with which we are acquainted excepting the pure spasm of gonorrheal, and other inflammatory affections, is a uniform contraction of the calibre of the canal, that might be denominated from its narrowness, the *cord* stricture. The vicinity of two such strictures produces in time a similar contraction of the interspace, which might with equal propriety be termed the *ribband* stricture. Such we believe, are seldom unaccompanied with spasm.

Another and more advanced state of stricture, is when a plait or fold of the internal membrane forms a ridge projecting equally from every side, or from one side only. In the former case the orifice must be centrical, in the latter a bougie passes readily over, under, or by the side of the fold. In either state they are rarely found to exist alone. The most common seat of the disease is a natural contraction of the canal behind the bulb, precisely opposite to the termination of Mery's ducts, at the distance of about seven inches from the lips. At this part a solitary stricture is occasionally met with, but seldom at any other. This circumstance has led to the opinion, that strictures nearer the glans or nearer the bladder, are sympathetic and secondary formations.

It is the opinion of Mr. Home, that this natural contraction is formed to prevent the regurgitation of semen from the bulb. In favour of this supposition he observes that " patients who have irritable urethras or strictures in that situation, find the part so much affected by connexion with women, that if it takes place oftener than once in the night, a spasmodic affection is brought on, inducing strangury."

Our author states two principal causes of stricture, inflammation and debility. As the latter is pretty universally admitted to be a consequence of the former, these distinct causes in a scientific view, reduce themselves to one. And we cannot help wishing that our author (who holds the disease to be *materially different* in its early stage, as it arises from the one or other of these causes) had given us a single argument for his opinion.

It is now no longer a question that the simple inflammation of gonorrhea and the remedies used for its cure are the most frequent exciting causes of this disease, and that it most commonly arises from them, only because they most commonly occur. These are points satisfactorily established by Mr. Home in direct contradiction to the opinions of Mr. Hunter. If injections however have been deprecated in books, let it be remembered that they have been abused in practice. They are in our opinion, a remedy used with too frequent impunity and too constant advantage, to sink in the esteem of the profession. Whatever occasions frequent or long protracted action of the muscles surrounding the urethra, as difficult micturition or coitus, becomes likewise a source of inflammation and strictures. Inflammation in such a part cannot be passive. Its first effect is to circumscribe the powers of self defence which the healthy membrane enjoys against the acrimony of the fluid it conveys; to diminish, if not to check entirely the secretion of its natural mucus.

Constant irritation keeps up a morbid excess of action, and speedily induces a change of structure. This may be either an alteration of original substance, or the deposition of a new one. It may be an effusion of serum or of lymph[*]. It is thus that we account for the thickened and callous stricture, and for the caruncles, and cicatrices, which writers for the last two centuries have described.

The symptoms of stricture, both local and constitutional, are now very fully before the public. Even science feels the influence of fashion, and the spirit of medical improvement has its periods of activity and torpor. The attention which Mr. Home has paid to the diagnostic characters of the disease under every modification, has probably left little to be added for centuries to come.

In the work to which we have alluded it is ably shewn, to how great a variety of morbid affections the extreme sensibility and extensive sympathies of this part give origin. Our experience does not furnish us with a more forcible illustration of the ill consequences of mistaking the symptom for the disease, and of the inutility of administering to the former with the hope of relieving it. For example, two of the most prominent in the train of local symptoms are a mucous discharge, and a nocturnal emission. The first is considered a gonorrhea, and astringent injections are employed. The second is esteemed a mark of impotence, and sexual commerce is recommended. Regular paroxysms of intermittent fever, irritability of the stomach and eruptions on the skin, have resisted for years the exhibition of appropriate medicines, and yielded to the use of the bougie. We now proceed to examine the merits of the caustic; and here our author shall advocate for himself.

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[*] In the cord and ribband stricture, this deposition is probably in the corpus spongiosum, and external to the membrane of the urethra. The adventitious membrane, on the other hand, is the production of its internal surface.

"There are two modes of treating this complaint; the one may be considered as a temporary or palliative cure, viz. the simple bougie, and the other as a permanent one, viz. the caustic, or armed bougie.

"With respect to the first, much has been said; many kinds of medicated, flexible gum, and metallic bougies have, at different times, been invented; each of them have been strongly recommended to the public by their respective authors; the particular virtues of each carefully enumerated, and all of them said to do wonders, by inducing a suppuration of the diseased part, and thereby removing the complaint.

"Experience has, however, shewn the inefficacy of these means in producing such a change; for no sooner is the use of the bougie discontinued, than the parts fall again into a state of contraction, which is almost always greater than the former.

"In most cases, I believe, the formation of strictures is equal on all sides, or the orifice of it is nearly, if not quite, in the centre of the canal; at least this conclusion is by far the most favourable to the use of the common conical bougie; for, from its shape, the effect which it produces must be by mechanical pressure, in the same way as you would remove the edges of any substance which is elastic, farther distant from each other, by the application of a wedge: if, therefore, the orifice is not in the centre, the bougie cannot act equally on all parts of the stricture, and, consequently, that side which is narrowest will be dilated first, and the bougie, passing on to the next obstruction, will necessarily leave a certain portion of the membrane in a contracted state.

"If, however, the orifice be in the centre, which, generally speaking, I believe to be the case; the conical bougie, by being daily passed as far as the opening will admit, and suffered to remain in the urethra, for one, two, or more hours at each time, and by gradually increasing the size of it, in many recent cases, may ultimately dilate the canal to nearly its original diameter; but the complaint must not, in all cases, be considered as removed, for the membrane, at that part of the urethra where the stricture was formed, being elongated in proportion to the state of contraction, and not being restored to its original healthy state, will be liable to fall into a contracted state again, upon the discontinuance of the bougie, and frequently without any apparent cause for it whatsoever.

"If we return to the bougie again, we shall, by perseverance in the use of it, often be able to dilate the canal to a certain degree; but, in every subsequent recurrence of the complaint, the contraction will be so much more increased, that we shall not be able to enlarge the orifice of the stricture as much as had been done on the former occasion. If the only inconvenience arising from this mode of treatment were the occasional return of the stricture, which, however, was always to be completely overcome by the same means, still that alone would have been a sufficient reason to have sought for a more effectual mode of obtaining relief; but, as the complaint was found to be liable to repeated returns, and, at each return, the strictured part was also ascertained to be in a more contracted state, it became a matter of serious consideration, whether the complaint did not admit a more complete and permanent relief, than any that had hitherto been experienced without a person's being in an almost constant use of the bougie.

"After having maturely considered this point, and having met with many cases of strictures, attended with very distressing symptoms, which would not yield to the use of the bougie, the late Mr. John Hunter suggested the application of caustic to them, with the hope, at least, of destroying so much as would enable him to prosecute and complete the cure, by the continuance of the common bougie, and in this he was by no means disappointed.

"Reasoning, *a priori*, upon the nature of this complaint, it is surely much more rational to expect that, if by the application of caustic, we can destroy that diseased portion of the canal, which is the cause of all, or any of the symptoms already described, we shall be more likely to succeed in producing permanent relief, than by simply dilating the parts by the use of the common bougie, whenever this can be effected, which, however, is not always in our power."

Dr. Andrews asserts in common with his friends, the justice of Mr. Hunter's personal claim to originality in the use of the caustic, and attempts to vindicate him from the charge "of a contemptuous silence of others," by the quotation of his note. Vid. Hunter 2d Edit. p. 128 and 129. We think, in tenderness, Mr. Hunter's real friends should tread lightly upon this ground. They cannot surely forget the pitiable dilemma in which he was left by that untameable *Huntero-mastix*, Mr. Jesse Foot. Admitting the fairest pretext that the fraternity of Madeira can urge, he stands convicted of disgraceful ignorance and libellous presumption. When Mr. Hunter's invention was published, it was not a discovery; and having found the real owner, he calumniated his reputation. The clumsiness of Wiseman's plan, says Mr. H. was probably the reason why he had not pursued the practice*. If we try Mr. H.'s plan upon this scale of his own contrivance, it will be merciful to let his friends finish the comparison.

The high and lateral puncture of the bladder we are informed are extremely objectionable, and "the operation per anum may be performed with as much certainty as if wholly directed by the eye." That all may be, and frequently have been performed with ease and safety, we well know. To the first, however, we object the necessity for the use of the canula, and the danger of its slipping from the cavity of the bladder as it subsides. Against the operation per anum the objection lies of an obstruction or deception from enlarged prostate, and the evacuation of the urine by the rectum, which must be subject either to the constant irritation of the canula, or to excoriation from the passage of the urine over its mucous membrane. If the operator is not intoxicated, the peritoneum in both cases, and vesiculæ seminales in the latter, are far enough removed from danger. If a stricture causing suppression from spasm or diseased prostate, is met with at the neck of the bladder, then, and then only is the puncture of that viscus indispensable; but as permanent stricture is always formed anteriorly to the prostate, freely laying open the membranous portion of the urethra with the knife will in a vast proportion of cases be found sufficient. We are aware however that this operation presents greater difficulties than are met with in lithotomy, from the want of such a director as the groove of a staff. Nevertheless, to persons well acquainted with the anatomy, and used to the operation of lithotomy, we are authorised to assert that it is attended with little difficulty, and commands the pre-eminent advantages of leaving the bladder unwounded, rendering the introduction of a canula unnecessary, and enabling us in most cases to divide the stricture with the knife. The following are our notes of the operation alluded to, as we lately saw it performed by a surgeon of high and deserved reputation. The subject of it was a healthy middle-aged man, who by a fall across a beam two years and a half before, burst his urethra. He had been troubled with strictures since the accident, which, notwithstanding the use of the armed bougie, grew continually worse. His urine previously to the operation was voided in wiry streams with much pain and straining, through fistulæ in perineo. A large triangular flap, the apex upwards, was raised in the integument opposite to the stricture, and left connected at the base. The fat which fills the interstices of the perineal muscles, much accumulated and indurated by disease, was removed in some quantity. The sinuses connected with the urethra were freely laid open, and the lower orifice of the ruptured canal discovered after some search, admitted a director to pass through it to the bladder. An elastic gum catheter with a stilet enclosed was now introduced at the proper orifice, and the stricture divided so as to expose its point; the same intrument was then pushed onward through the groove of the director into the bladder, the flap confined by sutures, and thus the operation was concluded. This instrument after being worn a fortnight, during which period the urine passed with freedom, and the wound in the perineum healed, became plugged with mucus and was withdrawn. A hollow metallic bougie was now substituted, and worn for ten days. This, however, the patient complained produced more irritation than the former, and the gum catheter was re-introduced and worn for a week. On its removal the urine flowed with little impediment through the proper channel, and this impediment was overcome by the occasional use of a common bougie, which was gradually increased in size. The patient is at this time perfectly free from his former feelings of distress, and a full sized bougie readily enters his bladder. As the mucous secretion may often require the removal of the tube at an earlier period, the difficulty of introduction may in part return. On this account it would be desirable, if possible, to enclose one tube in another as the wire is enclosed in a canula, so as to admit of removal and replacement without risk of losing the passage. But the size of the instrument would in this case render its use very questionable.

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* The discontinuance of which, by the bye, he no where insinuates.

Where the obstruction is of long standing the parts have become so thickened and callous, and retain so little of their original structure, as to require much knowledge and coolness on the part of the operator. The great difficulty in these cases where the passage is partially obliterated, is to discover what remains of it. The expulsion of the urine will often lead to this. A young operator, who had only heard the process described, would be exposed to no just reproach if he abandoned it in despair.

Dr. A. presents his readers with the minute detail of an interesting case entitled "suppression of urine from strictures," (vid. p. 43) in the treatment of which he was aided by his friend Dr. Adams. When we read of a suppression suddenly induced in a patient of very irritable disposition, where, after the use of the warm bath, topical fomentations, and opiates, the parts having become tranquil and free from spasm, a flexible gum catheter went "with great ease into the bladder," where by the aid of antispasmodics the instrument could be passed twice daily "with perfect ease, and as freely as if there had not been any obstruction," we are somewhat surprised to learn, that *from the commencement of the attack* it was intended to take up the cure by caustic as soon as the disposition to spasm had subsided. Although a considerable quantity of urine passed by the side of the catheter as it lay in the bladder, and all the symptoms of suppression re-appeared with equal violence nearly two years afterwards from the irritation of piles and the imprudent retention of urine, yet strictures are stated to have been overcome at 5, 6, and 7 inches from the orifice, by a few applications of the caustic! Who can doubt that such strictures were spasmodic, and that the bougie *unarmed* would have been equally efficacious. But be this as it may, other means having proved sufficiently so to allow the *perfectly easy* passage of the bougie into the bladder before the caustic was taken up, and the suppression of urine being fully as complete after its best effects had been produced, the obvious inference is, that it neither relieved the one attack nor prevented the other.—The doctor's resolution to causticate before the permanent disease (if it ever existed) could possibly be ascertained to exist, savours much of the simplicity of Dr. Sangrado's principle. In his ardent devotions to the Lunar nymph, while the "observations" were yet in embryo, the doctor seems to have adopted the maxim of the Epicurean philosopher, "*habeo, non habeor.*"

Before quitting the subject of suppression, we shall take the liberty of observing that more depends upon sheer manœuvre in the introduction of the catheter than in any other surgical operation; this observation we are confident will be confirmed by every surgeon of experience. It is generally recommended that no force be exerted in the attempt, and if the recommendation be taken, it is only in cases of pure spasm that the operator has a chance of success. In this case the least injury that violence can commit, is the aggravation of the symptoms which it was intended to relieve. We certainly do not remember to have seen the urine drawn off where the obstruction arose from a permanent cause, without the application of such a degree of force as could alone be justified by an intimate knowledge of the canal and long experience of its diseases. It is seldom that any difficulty presents itself until we reach the curve of the urethra, and if the concave face of the instrument has been gradually raised from the abdomen and the penis is at the same time put well upon the stretch, there is as rarely any difficulty here. Much more of the tactus eruditus is requisite to perform the half circle, and keeping the point free of folds to throw it at the same moment under the arch of the pubes. This fantastic twirl of the instrument always appeared to us to be a relic of the nick-nackery of the chirurgeon.

The depression of the handle of the instrument should be continued until the angle which it forms with the abdomen, is become as much obtuse as it was at first acute. This method of elevating the point of the catheter over the obstructing cause we have so often found successful, as to be induced to think that in a great majority of cases it is situated upon the under surface of the canal. If in the diseased enlargement of the prostate gland depression of the handle be not carried to still greater extent, any considerable force will penetrate the third lobe, which in these cases is found to project like a valve over the mouth of the bladder.

Our author cites some of the principal objections of Messrs. Bell, Foot, and Whately, and summons up once more the troubled ghosts of Saviard and Dr. Rowley. His remarks, however, are for the most part confined to the publication of Mr. Whately. To this gentleman's observation that in Mr. Home's instances the remedy proves often worse than the disease, Dr. A. replies, that Mr. H. has given without exception the severest as well as the mildest cases which his extensive practice afforded, and that the rigour, suppression, and fever exist in many cases before the use of the caustic; "the constitution having been brought into a considerable state of irritation by the continuance and violence of the complaint, to which is probably added a very intemperate mode of life."—To the insinuation that the false passage which Mr. W. found after death in the urethra of Mr. H.'s patient had been formed by the caustic, Dr. A. opposes the greater probability that the patient had been previously in the hands of some less skilful practitioner, than that he should have fared so ill in those of Mr. Home. After quoting from the latter a remarkable case of false passage occasioned partly by a calculous obstruction, and which our readers will probably recollect, Dr. A. concludes with the following observation.

"From the history of this case it is evident, that the false passage was formed by the use of too small a caustic bougie in the first instance, which not being able to be applied to the whole surface of the stricture, had only destroyed the under part of it; on which account the resistance made by the stones had forced the bougie out of its proper course."

"The armed bougie should be as large as the canal will readily admit for two obvious reasons; viz. to prevent the caustic's being applied to a part of the urethra where there is no stricture, by which a false passage might be found, and secondly to prevent any stricture which may be in a less contracted state from escaping notice, in which case the patient would not obtain relief, and the treatment would naturally fall into discredit.

These and similar remarks are followed by two cases in point, and are, we think, substantial objections to the use of the small caustic bougie so strenuously recommended by Mr. Whately. Hemorrhage Dr. A. attributes not to the exposure of the cells of the corpus spongiosum, but to the casting off of the slough before inflammation has united the sides of the injured vessels. He reminds us that bleeding may be induced as well by the simple as the armed bougie, and that "cases of violent bleeding are met with when neither caustic or bougie have been used!"

On this subject we may observe, that hemorrhage, although the immediate consequence of disorganization simply, depends in point of extent and frequency upon the modus operandi of the destructive agent. It would be altogether incorrect to imagine one escharotic more exceptionable than another on this ground, because it had a more diffused action. The preparations of arsenic and mercury which destroy by their excessive stimulus, command as great an advantage over the chemical caustics in lessening the danger of hemorrhage, as these in their turn derive from their circumscribed and local action. Where a vessel is opened or a slough is formed by the operation of caustic, inflammation is wanted to increase the natural powers of arterial contraction, and to induce a process of adhesion; and it is of no importance that caustic is in itself styptic when it has produced a separation of parts. When on the other hand the surrounding vessels have partaken of that excess of action which has terminated in the death of the centre, they are provided with all the means which nature has contrived to administer to her own relief.

It is thus that we explain the long continued hemorrhages which so frequently follow its employment. In reply to the objection that the cure by caustic is not permanent, our author admits that violent local effects will produce such a change on the urethra, as renders it liable to fall again into a state of contraction, but adds, that his experience furnishes but one case of this description, and that it may on most occasions be overcome by frequently passing a bougie. In the case referred to, the strictures formed at their original situations, a proof he thinks that they were not fresh contractions in consequence of the use of caustic. The tendency of parts once diseased to become so in future, is a pathological fact of very extensive application. The inflammatory affections of the mucous membranes furnish a familiar example. But it is not to this law of morbid susceptibility that Mr. Whately refers the recurrence of strictures, but to some unaccountable tendency to reproduction, of parts destroyed. The return of the disease, it must be evident to common reflection, will be most frequent where the least organic alteration is induced. We have yet to learn that the sloughs of a disorganized membrane like the fabled ashes of the Phœnix, are but the nidus of a new formation; and that the regeneration of morbid structures is subject to the same unerring laws as regulate the natural processes of reparation and growth.

When we look upon the long and frightful catalogue of the anticausticators, (or, as geology has given up the term, of the *anti-vulcanists),* we have no difficulty in believing that the practice has suffered more from unskilful management than from ill desert. The danger of an instrument in itself is one thing, and in its use another. In the first view, the caustic is safe, mild, and efficacious; in the latter it is often hazardous, violent, and unavailing. We have seen its good effects so often and so strongly marked, as rather to feel surprised with its former abandonment than its present popularity. We have little respect for the physical powers of those wholesale dealers in clamour and invective, who glut and sicken the grossest appetite; but the petty retailers, making a shew of candour that they do not possess, are objects of our serious terror and aversion. There is one point upon which considerable differences still exist, viz. the cases that require its employment. Mr. Hunter never had recourse to the caustic where he could reach the bladder by a bougie. Dr. Andrews follows the plan of Mr. Home, which is to use it in all cases to the cure of which the bougie is unequal, and probably we think in *a few more.* Mr. Whatley in direct contradiction to Mr. Hunter, never uses it before passing small bougies.

The proposal to substitute one species of caustic for another, savours, we think, a little of the spirit of change without difference. It is a practical minutia, not a variation in principle, and therefore does not affect the question. In one case where kali purum was tried according to the given directions, it appeared to us to command none of its boasted advantages. We cannot, however, be ungrateful for the economic proposal to combine in a single operation the *cure of strictures and the manufacture of soap!* To a long list of cures by caustic, cases are added of strictured œsophagus, and a plate that well represents the disease. Here, too, the Dr. administers and recommends the sovereign remedy. Its merits, in the whole extent of its application, cannot be fairly appreciated from promiscuous cases and partial observations; and in this, as in other questions of controversial magnitude, the present age may hear the pleadings, but posterity will pronounce the verdict.

ART. VI. *Observations on Morbid Poisons, Chronic and Acute.* By Joseph Adams, M. D. F. L. S. 4to. pp. 406. 1807.—Callow, London.

A CERTAIN interval of time, it is generally acknowledged, must be permitted to elapse after the removal of every distinguished public character, in order that a correct estimate of his peculiar defects and excellences may be formed. During the life of such individuals, and also immediately subsequent to its conclusion, the force of prejudice, operating under various forms, must necessarily corrupt the decision. Friendship, produced and fostered by the powerful influence of personal kindness and flattering notice or attention, on the one hand; and antipathy, excited by personal interference or invidious feelings, on the other, alike tending to destroy that cool and dispassionate condition of mind, which, in a deliberation so momentous, ought ever to prevail. The necessity of this attempering interval being admitted, it may become a question, what ought to be its duration, in order completely to secure the important end proposed? To this question it is difficult to offer a definite answer, because it is in a great degree a question of circumstance; of circumstance dependant on the proportion of attractive or repulsive qualities in the character of the individual. If, however, we were required to advance a general principle, we should say, that the time alluded to ought to be long or short, in the direct ratio of the popularity previously enjoyed. Whether, upon this principle, the present is the proper time, to sit in judgment on the scientific claims of an anatomist, physiologist, pathologist, and surgeon of the highest celebrity, whose opinions and praises constitute the largest portion of the work which we now propose to examine, we shall not presume to decide. We know, and can assure our readers, that in the investigation of the doctrines of John Hunter, we can proceed without encountering any of the difficulties already stated; and shall therefore, we hope, be protected from the scientific idolatry of Dr. Adams,—his unqualified and universal panegyrist; and the invidious hostility of Mr. Foot,—his illiberal and malignant defamer.

The majority of those who have perused the first edition of this work, remain, we suppose, unacquainted with the circumstances which led to its publication. In the present edition this information is supplied in the dedication. We are there told that the author was the pupil of Mr. Hunter, and that he was "favoured with many opportunities," as a pupil or friend, we suppose, of oral communication. If, in consequence of this information, they should expect to find, either in the modest octavo of Joseph Adams, surgeon, or in the elaborate quarto of Dr. Joseph Adams, any thing which, without such privileges could not have been displayed, we can promise them a serious disappointment: at least a perusal of both editions, which we may be permitted to term assiduous, has directed us to very little that has not been long accessible to the public. These advantages (including "much assistance from others," seldom excepting in copious quotation to be found,) "which *ten years* have been spent in improving," appear to us essentially to have injured the work. They have occasioned, we suspect, that undiscriminating admiration of every performance of Mr. Hunter, which pervades both editions; and have produced, we apprehend, that reverential feeling which has sanctified even the errors of this philosophical idol.

But time, which determines the real value of all opinions, is, we are told, giving extension and authority to those of Mr. Hunter: "*It is easy to perceive that Mr. Hunter's opinions are every day gaining ground, and even his language coming into more general use*[*]." And again, "*It is however with much pleasure I find that the opinions of this great man are insensibly gaining ground. With his opinions his language necessarily makes its way*[†]." When we consider the quality of many of these opinions, and of much of that language, we must express our regret that this should be the case, and our consequent hope that the conclusion may rest on the exaggerations of partiality. Our apprehensions respecting their extension, is not however increased by the *work on Morbid Poisons;* since, unless they are adopted more rapidly and extensively, in proportion as they are less intelligible or less understood, (which we are not without fear is often the fact,) we do not perceive any probable mischief from this source. Yet should there exist any foundation for the assertions which we have repeated, it seems absolutely necessary to direct inquisitive minds, by a few remarks, to the revision of these subjects.

Two instances are particularly insisted on as proofs or illustrations of the superior accuracy of the language of Mr. Hunter; *hereditary disposition* substituted for *hereditary disease*, and the word *coagulating* instead of *coagulable*, applied to the lymph or fibrine of the blood. By the first it would appear that Mr. Hunter had decided an important pathological question; and by the second, had announced a fact, which, if previously known, had not been intimated by the term used for designating it. By these etymological attempts, very little, we think, has been achieved. No controversy has existed or could have existed on the question, whether diseases absolutely formed were transmitted from parent to child? but, whether such imperfections of structure or constitution did occur in consequence of the condition of one or both parents, as that similar diseases would appear in the offspring, no cause having operated, sufficiently powerful to have produced them in persons not having originally such imperfections. Those who have contended that phthisis, scrophula, or mania, are hereditary diseases, could not surely be supposed to maintain that a child, in order to *inherit* them, must be born with ulcerated lungs, enlarged jugular glands, or disordered intellects; and therefore we cannot perceive how, by this change of language, that agreement amongst writers of opposite sentiments, or that addition to "the stock of our knowledge," can have been produced, of which Dr. Adams speaks in reference to Mr. White's Essay on Struma *. It is not necessary for the support of the affirmative of this question, that a generation should never pass away without some appearance of the disease; for into medical as well as legal descent, contingency, such as a proper exciting cause and survivorship, must be admitted. When Mr. Hunter asserted, during his examination in the trial of Captain Donellan, "*that there was no such thing as an hereditary disease, though a disposition to disease might be hereditary,*" we are not surprised that the whole deposition was materially invalidated †; it would in that place be instantly discovered, that Mr. Hunter had seized a forensic instrument, known by the name of *a quibble*; an instrument not expected to be wielded by chirurgical hands, but when dexterously managed by appropriate practitioners, possessing great capabilities for lopping off redundant or troublesome information. In regard to the word *coagulating*, we may remark, that it is defective in not expressing the meaning which it was intended to convey. If the participle present of every verb uniformly denoted the condition of a body, resulting from the operation of a self-exerted power, the alteration would only be chargeable with assuming as true, what is yet by no means proved; but it may also be taxed with incompetency. A moving body, for example, often signifies a moveable body, which moves in consequence of a force exerted ab extra, as well as one in motion by the exertion of its own internal powers: consequently, coagulating lymph may be understood to signify lymph which is coagulable in the very act of coagulation, as well as the change occurring through the agency of an internal self-exerted cause. Innovations in language, liable to objections still more serious, have been introduced into the writings of Mr. Hunter, and defended by Dr. Adams, which we shall soon have an opportunity to notice. At present we shall not enlarge on this subject, excepting to express our regret that these gentlemen, whilst they wrote, had not been fully sensible of the truth of the following remarks: "It has been my wish," says Mr. Pearson, "to avoid as much as possible that fondness for novelty which delights in petty reformation; to encourage such a taste is the indubitable characteristic of a minute mind, which, by endeavouring to appear the more learned, only becomes the more troublesome."

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[*] Adams: preliminary remarks, p. iv.

[†] Ibid, p. xxiii.

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* Morbid Poisons, first edition, p. 8.

† Ibid, second edition, preliminary remarks, p. 3.

In the preliminary remarks, in order to introduce with greater effect Mr. Hunter's pathological opinions, his extraordinary, nay, his almost exclusive correctness in physiological research, and his tenacious, undeviating adherence to the inductive logic of Lord Bacon, are strongly maintained. To confirm the high encomiums which are lavished on Mr. Hunter, and to prove that " he has taught us how to explore nature," with the means of avoiding the errors of those who have preceded and followed him, his opinion respecting the life of the blood, the inquiry into the nature of hernia congenita, and the discovery of the digestion of the stomach after death, are separately detailed and illustrated.

That Mr. Hunter revived the Mosaic opinion of the life of the blood, is generally known; that it was an opinion also held by Hippocrates, who says, " omnia animantur in corpore animato," and by Harvey, cannot be denied. Into the controversy that ensued, (that unprofitable war of words which may be prolonged to the last struggles of speculative folly, unless greater precision in the use of terms is observed,) we do not propose to enter. We are inclined to admit that this fluid exhibits appearances, which distinguish it from fluids of an ordinary kind, but we must deny that the change from dead to living matter might, as Mr. Hunter undertook to shew, be found in the blood*; it must take place nearer to the original substances introduced for the increase or reparation of living parts, and may be transferred to the chyle, or even perhaps to the pultaceous mass passing out of the pylorus; the impossibility of imitating this substance by any process conducted out of the stomach, and the spontaneous coagulation of chyle observed by Hallé†, being proofs of the same kind with those of Mr. Hunter: nor whilst allowing, as every one must who has thought upon the subject, that the blood, as a part of the animated system, possesses properties distinct from those of its constituent particles, can we prevail upon ourselves to grant, that it is endowed with intelligence; yet something of this kind seems to be claimed for it in the following sentences:

> " I have now considered the circumstances under which the blood coagulates, and shewn that none of them alone, or all of them combined, induce the blood to coagulate. My opinion is, that it coagulates from an impression; that is, its fluidity under such circumstances being improper, or no longer necessary, it coagulates, to answer now the necessary purposes of solidity. This power," he continues, " seems

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* Morbid Poisons, prel. rem. p. 16.

† Fourcroy, Systeme des Connois: Chem, Tome X.

to be influenced in a way in some degree similar to muscular action, though probably not entirely of that kind; for I have reason to believe that blood has the power of action within itself, according to the stimulus of necessity, which necessity arises out of its situation*."

The cause or causes of that spontaneous separation, as it is hypothetically termed, which occurs in the blood, experiment has not yet disclosed, nor can we expect that it ever should, such verbalism as that we have just quoted, being accepted as the ultimatum of research. If the impression on which it is said to depend, be physical, we may still hope to detect it; if spiritual, we must rest satisfied with knowing it in its effects alone. This impression, it seems to us, must be understood to be of the quality proceeding from intelligence; and that the blood must have a power of *thought*, as well as of action: we are told that

"A total stagnation of the blood, while the body is alive, as in a trance, or where the circulation has been stopt for several hours, as in the case of persons apparently drowned, does not make it coagulate; yet where there are no actions going on in a part, if the blood stagnates for a much shorter time than in a trance, it will be found coagulated, as in mortifications; but then this coagulation is *to answer a good purpose*, and arises from *necessity*, which appears *to act as a stimulus in disposing* the blood to coagulate†."

That Dr. Adams, so delicate in respect to every logical indiscretion, and so ceremonious in introducing to us upon all occasions his knowledge of induction, should not only have been able to permit these passages to remain without reprehension, but should actually have made one of them a subject of praise, must exceed the conception of every one, not like ourselves, thoroughly acquainted with his blind devotion. We have not sufficient room to offer all the strictures which these and many similar passages suggest; but we must be allowed to say, that we can scarcely imagine any thing more fatally capricious than this fluid which ranks so high in animated beings, nor an endeavour to confound, more determined, than appears in thus bringing together *drowning*, *mortification*, *and a trance*.

When Mr. Hunter, or in former periods, Dr. Adams, in removing a tumour had divided an artery, the danger from hæmorrhage being very great, the stimulus of necessity must have been sufficiently urgent to have produced, we should conclude, immediate coagulation; but no such thing;—the blood at the extremity of the divided artery, where its fluidity is no longer wanted, and where its solidity would certainly answer a very good purpose, rashly makes its escape, and never discovers, (but when too late to be of any service), that the *necessary* purposes of solidity might have been accomplished elsewhere, much better than on the floor or in a bason. It is true, indeed, that if the velocity of the effluent blood should be retarded, by means which casually occur, and which in the ingenious experiments of Dr. Jones were intentionally produced, the fluid obtains such an opportunity for delay, (affording time, we conclude, for *proper reflection* on the *circumstances* and *necessities* of its situation), that it does gradually take on the form best calculated to close the orifice. Coagulation commences on the fibrous portions of cellular membrane, projecting into a space of larger dimensions than the artery itself left by the retraction of its divided extremities, until at length what is termed the external coagulum is produced. This coagulum gradually acquires a more firm union with the inner surface of the higher orifice of the artery, and assisted a little by the internal coagulum, secures the system against the farther effusion of blood*. Had we not been fully aware of Mr. Hunter's determination to *intellectualize* the blood, we should have been astonished to find a surgeon bringing forward mortification, in which there is a greatly diminished action, or absolute cessation when in the highest degree, in a part only, as being in a state completely similar, excepting the possession of the stimulus of necessity, to the same portion of a body drowned, or in a trance: he must have observed, that though there is a loss of action in a gangrenous, and no action at all in a sphacelated part†, yet there is an action throughout the system, and this, which gives rise to the subsequent phenomena, destroys entirely the analogy. The retardation of circulation will admit of the coagulation of which Mr. Hunter speaks, even nearer to the heart than the part immediately concerned, and the sphacelated part no longer possessing a vital union with the system, its removal is effected by operations resulting from established laws of the animal economy, like any other extraneous substance; whilst the separation taking place by the formation of pus, must occur in a part where blood no longer passes, and consequently where no hæmorrhage can appear. We may assert, in the language of an author, who can describe without theorizing, and theorize without the perpetual introduction of metaphysical nonentities, that in mortification "the separation of the dead part from the living is a natural process, and is explicable upon known philosophical principles[*]."

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* Hunter on the Blood, p. 25. † Ibid, p, 24.

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* Vide Kirkland's Distinction between Gangrene and Sphacelus.

† Jones on Hæmorrhage, Expr. 8 and 14, pages 38 and 45.

Before we dismiss the subject of coagulation, which, from its importance amongst the speculations of Hunter, we have been induced particularly to notice, we may briefly express our surprise, that the blood has been endowed with a vital principle to effect a purpose so seldom requiring its exercise. Mr. Hunter has indeed said, "that life could not go on without the coagulation of the blood; for as all the solid parts of the body are formed from the blood, this could not take place if there did not exist in it the power of coagulating[†]." That the formation of new solid parts is a simple act of coagulation, cannot be admitted, until it has been shewn that muscle, nerve, and bone are nothing but lymph coagulated, and that no new arrangement of the constituent particles has occurred, by which solidity, as well as a different chemical combination may be produced. Had we been required to assign an office to the "*materia vitæ*" of the blood, we should have employed it as Van Helmont has done in keeping it fluid and homogeneous. Lastly, that we may display in the most compendious manner, the perspicuity and precision of Mr. Hunter's ideas, we shall permit him *four* times to pronounce what coagulation is:—"It is a new species of combination[‡]." "It is a something in the blood itself[‖]." "It is an operation of life[§]," and "It is particle uniting with particle, by the attraction of cohesion, which in the blood forms a solid[¶]."

Judging from the tone which resounds through all that Dr. Adams has written respecting *hernia congenita*, we infer, that he considers Mr. Hunter, by his researches, to have gained a complete triumph over all the ignorance which had ever obscured the subject; if we adopt implicitly the history of the disease delivered by our author, and he proposes to trace it "in a manner no one can object to[**]," we must certainly grant that every thing is due to Mr. Hunter. We feel, however, disposed to attempt another sketch of the progress of the discovery, in order to give our readers an opportunity of ascertaining in this unexceptionable history some *little* circumstances affecting the claims of others, have not been overlooked or suppressed. By Morgagni we are informed that Boscus, in his Sepulchretum, states himself to have seen the vaginal coat of the testis, (which he calls a process of the peritoneum,) so much dilated from its origin to its fundus, by a portion of intestine, that two fingers were easily introduced into it*. In the Memoirs of the Academy of Sciences for the year 1701, a case is related by M. Mery, in which, on opening the scrotum of an adult subject, a portion of omentum was found adhering to the testicle, and a portion of intestine had also descended into the tunica vaginalis. Mery supposed that these parts of the contents of the abdomen had descended through an opening, in this case connecting the two cavities, in the same manner in which he had observed them to communicate in all the animals which he had dissected.

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[*] Pearson's Principles of Surgery, art. cciv. p. 119.

[†] On the Blood, p. 7.

[‡] Hunter on the Blood, p. 21, (note)

[‖] Ibid, p. 24. [§] Ibid p. 26. [¶] Ibid p. 26.

[**] Morbid Poisons, p. xvii.

In mentioning the opinion of Mery, Morgagni says that he would have adopted it had not the same appearance so frequently presented itself to him, and had not Boscus said that "he had seen it very often, and that it might easily be seen by all:" for these reasons Morgagni could not admit that it arose merely from a peculiarity of structure; and he accepts therefore the idea which Mery had before successfully combated†, that the occurrence took place in consequence of a rupture of the proper herniary sac and the tunica vaginalis testis.

Mr. Sharp, in the year 1748, (and here Dr. Adams's history commences,) observed the appearance described by Mery, which he immediately represented to Dr. Hunter, who was so far a stranger to the circumstance, that when asked "if he had ever seen intestine in the same bag and in contact with the testis," replied, that he "never had found it so, and did not think it possible‡"—when convinced of the fact by inspection and by a similar case which occurred to himself, Dr. Hunter could not explain it otherwise than by the aid of the improbable double laceration before noticed, and in this explanation he was followed by Sharp and Cheselden.—Until the year 1755 when Baron Haller's opuscula pathologica were published, (which although *seven years* after the transactions last mentioned, Dr. Adams states to have been "about the same time,") Dr. Hunter continued to offer the same explanation: on reading in that justly celebrated performance, the observations on hernia congenita, "it struck my imagination," says the Doctor, "that the state of the testis in the fœtus, and its descent from the abdomen into the scrotum, would explain several things concerning ruptures and the hydrocele, and particularly that observation which Mr. Sharp had communicated to me:" he then imparted his ideas to his brother, and "both were convinced that the examination of the facts would answer their expectation*." Now we cannot discover what the imagination of Dr. Hunter could possibly have to do in this instance; for after the exercise of correct vision in the perusal of the words, we think that the most simple faculty of perception alone was required, to recognize in the passage annexed, the whole interpretation of the phænomenon. "In fœtu enim testes in cellulosa tela lumborum sedent proxime renum. Descendent inde sensim et in scrotum adveniunt, semper retro peritonæum &c. Causa hujus progressus videtur in respirationis vi et in musculorum abdominis potestate poni. Herniarum ni fallor, congenitarum, modus hinc elucescit, que generantur. *Patulus est processus peritonæi* &c. Cum autem his in corporibus testes eodem cum intestinis sacco omnino contineantur, nihil est singularis sive inexpectati, si ea in apertum saccum a levi vi depressa fuerint. Annon id suadet notissima observatio longe plerosque herniosos in ætate infantili id vitium contraxisse†. The origin of the testes within the abdomen accounts for the existence of the aperture, as well as its necessity, where the scrotum is to be the future residence of the testes; and this aperture may be used either as a part of the iter testium, or for the escape of a portion of omentum or intestine protruded first, or following in consequence of subsequent pressure, before the annular space is completely closed. To Dr. Haller we are certainly indebted for the first explanation; and he might unquestionably have taxed the Hunters with plagiarism, much more justly than they were enabled to tax Mr. Pott with this fault. In the progress of this history of congenital hernia, some mention we think, ought in justice to have been made by Dr. Adams, of the use to which Haller applied his knowledge of the fœtal position of the testis; a fact, which if not new to Dr. Hunter and his brother in the year 1755, appears to have existed in their minds insulated and useless: instead, however, of mentioning this very valuable inference, Dr. Adams, as if determined to make a diversion in favour of the subject of his eulogy, immediately begins to state and refute the mode by which Haller and Pott have attempted to explain the descent of the testicle. That the action of the respiratory organs in whatever manner exercised, is incompetent to the effect, will be easily understood, and that if the descent take place before the child has breathed, (frequently though not always the case,) they cannot have even an opportunity of exerting their influence, will not be disputed; but have we, it may be asked, from knowing that the testicles are abdominal viscera no longer than until the end of the eighth month, obtained a perfect knowledge "of the mode in which the testicles descend?" It may certainly assist in determining what powers have not been employed, but cannot, we conceive, be applied with any success, in exhibiting those which may have been efficient. Of this Dr. Adams seems to think otherwise, as the following account of what has been done by Mr. Hunter, delivered with suitable adaptations, in the language of Lord Bacon, will shew.—"This last task was reserved for one who considered that though throughout nature nothing really exists but individual bodies having simple individual actions according to a law, yet, in every theory, that law and the tracing, exploring, and explaining of it, is the foundation of science and experiment. Rejecting therefore the syllogism which never stops to obviate objections, and every induction which would not apply to the intermediate as well as the principal axiom, we find him attentive only to facts, their series and order, and forming no conclusions as long as any objections remained. By a close observation of the state and situation of the testis in every age of the fœtus, he discovered the *time* which led to the *mode* in which the testicles descended, not only without the accidental aid of weight, posture, or temper, but without the danger of the intestine descending with it. In a word, he traced nature throughout every part of the process, and shewed she had as regularly provided for this change, and as much by laws suited to the actions produced as she does for the changes from the fœtal to the state of respiration, from childhood to puberty, from the virgin to the impregnated uterus.*" Who, on perusing this laboured annunciation, would not expect to be immediately conducted to the most clear and convincing elucidation of the occurrence: as we proceeded we expected to see it displayed in the succeeding paragraph, but unfortunately for our expectation the doctor there renews his favourite attack on the awkward mechanical attempts of Haller: in the observations of Mr. Hunter, printed in the medical commentaries for the purpose of criminating Mr. Pott, or of vindicating the reflections thrown upon him by Dr. Hunter, we sought for this brilliant explanation in vain; but on carefully looking over the essay as it appears in the treatise entitled "observations on certain parts of the animal economy,"—we found, yes, with astonishment we found,—that it is by "the stimulus of perfection*" that the testicles are impelled to descend. We do not wish Dr. Adams to understand that we think he has omitted this explanation because he was at all *ashamed* of it; we do assure him that there is nothing in the whole work which, in our opinion, could in the slightest degree countenance such a supposition; but we conclude that having taken *all* his information from the medical commentaries, he had not become acquainted with that which we have stated. Whether the *fuga vacui*, which, as employed to account for the ascent and suspension of mercury in the barometer, Dr. Adams very properly reprobates, is more reprehensible than the *stimulus of perfection,* which as proposed by Mr. Hunter for explaining the descent of the testicle he must be understood to applaud, we leave for the decision of our readers.

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* Morgagni de Sedibus et Causis, &c. Epist. 23.

† Mem. de l'Acad. des sciences, ſur les hernies; 1701.

‡ Medical Commentaries, page 70.

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* Medical Commentaries, p. 72.

† Opuscula Pathologica. Lausanne 1755, p. 58.

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* Morbid poisons; preliminary remarks, page xix.

"The description of the situation of the testis in the fœtus" anatomically considered, we can with sincerity and pleasure commend: the minute, distinct, and accurate manner in which all the cirumstances are delineated, cannot be too much praised, and we may advantageously recommend it as a model for conducting similar investigations; yet, even amidst all this excellence, there appears an imperfection in the account of the cremaster muscle.—"The cremaster or musculus testis" we are told "appears to be composed of the lower fibres of the obliquus internus and transversalis muscle, in the fœtus turning upwards instead of going across to the pubis, and spreading upon the external surface of the gubernaculum immediately under the peritoneum; it appears to be lost in the peritoneum a little way from the testicle.†" At the bottom of the next page we meet with the following account of the same part: "in the human fœtus, while the testis is retained in the cavity of the abdomen the cremaster is so slender that I cannot trace it to my own satisfaction either turning up towards the testis or turning down towards the scrotum, yet from analogy we may conclude that it passes up to the testicle." He had previously remarked that "in those animals where the testicles

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[*] On the Animal Economy, page 18.

[†] Ibid. page 6.

change their situation, the cremaster muscle has two very different positions in the fœtus and in the adult; the first of these is the same as in those animals whose testicles remain through life in the cavity of the abdomen"--now the sameness here stated may be satisfactorily established; for in the fœtus Mr. Hunter has said that the fibres of the cremaster could not be seen either within or without the abdomen, and Cuvier has informed us, that in those animals whose testicles remain through life in the abdomen, there is no cremaster muscle*. We also perceive some appearance of difficulty in assigning the office of the ligamentum testis, or as Mr. Hunter terms it, gubernaculum testis, which he has very well described: he saw that it existed in animals, such for example as the hedge-hog, from the abdomen of which the testicles never escape; and he was perhaps aware, that "the stimulus of perfection," or the "provision for this change by laws suited to the action to be produced," ought to guide, as well as move towards the orifice, the descending testicle: yet in the explanation of the second plate, he calls this part "the gubernaculum or ligament which bound and *conducted* the testis to the scrotum." He likewise takes advantage of a mechanical operation, by making the pyramidal or wedge like head of the ligament "not only to direct, but to make room for the testis which must follow it." The power which acts upon the wedge must be that, whatever it may be, which propels the testicles, for no other power is discoverable; consequently the testicle would be equally capable, without the assistance of this extension, to make its way through the aperture: this will become still more obvious by considering, that the enlargement noticed cannot be equal to the whole enlargement required, the testicle being a prolate spheroid, the shorter diameter of which is represented to be equal to the width and superior to the thickness, of the head of the gubernaculum.

Here it may not be improper briefly to notice the alternate abdominal and scrotal situation of the testis of several animals, as described by naturalists, of which *the language* of Mr. Hunter furnishes an easy and intelligible explanation. The stimulus of perfection "makes the testicles assume the disposition which is requisite for their descent†," and "the stimulus of imperfection" we may infer, must be employed in producing an opposite movement; or we may conclude that the state of the testicle then "becomes similar to that in a simple accident, viz.

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* Lecons d'Anatomie comparee; Lecon xix. tom. v.

† On the Animal Economy.

a consciousness of imperfection is excited which produces the action of restoration :*" by this restoration to the former position, the organ now imperfect or disqualified, remains in appropriate concealment.

The opprobrium affixed by Dr. Adams, in this part of his preliminary remarks, to reasoning on mechanical principles, and to Haller in particular, for his employment of them, certainly is not altogether merited. Many no doubt, seduced by their simplicity, have introduced them into physiological speculations where they ought never to have appeared, but in one instance, selected and descanted upon with much warmth by Dr. Adams, this impropriety does not so glaringly appear. The removal of the thymus gland, we allow could not be accomplished by the " dashing and overgrowing " (or as we should translate the words elisa et aucta,) the pressing and increasing organs which are contained with it in the same circumscribed cavity; but absorbent vessels having been provided, this pressure and increase of size, may so far augment their action, as to contribute to the effect, if not wholly produce it. Dr. Adams need not surely be reminded of the removal of parts of the vertebræ by the dashing and accumulating blood in aneurismal enlargements of the Aorta, nor of the assistance which the surgeon continually derives from pressure, in the treatment of tumours. This, Mr. Hunter whose authority is paramount to all other considerations, maintains twice in the same page: in the second case, speaking of a considerable tumor produced by an accident, he says " I then directed a slight compress to be applied, after which the tumor evidently diminished much faster than before.†" On account of the absence of visible muscularity, and the presence of so many valves in the absorbent vessels, it has been contended by a late eminent anatomical teacher in this metropolis, that absosption is carried forwards entirely by pressure, exerted upon their yielding coats, and that without such pressure, the utility of the absorbent system would cease. Although this may not be true to the full extent, yet the efficacy and necessity of pressure in promoting absorption, cannot be denied, and therefore it must be admitted as one of the causes, why Haller, who has only mentioned *one cause,* should have had directed against him, the often-quoted reflections of Sir Isaac Newton against *the multiplication of causes,* we are unable to discover. In behalf of mechanical explanations we may be permitted to add, that in consequence of employing sensible means they possess one advantage of being easily established or refuted; this has been illustrated by the facility with which even Dr. Adams has been enabled to controvert Dr. Haller; but in defence of the free use of multiplied stimuli, such as the stimulus of necessity, of relaxation, of death, of perfection, of imperfection, and the like, reviving the dialectic jargon, and the mysticism of occult causes, nothing we think can be offered. In connection with these observations, we recommend to Dr. Adams a serious and attentive perusal of the following extract, which he will do well to remember when returning to the writings of Mr. Hunter: "We ought carefully to distinguish between a true physical cause, and those which may be termed metaphysical; a knowledge of the former constitutes real science, and conducts to useful practice; to rest satisfied with the latter, is to be contented with empty figments, and barren speculation. If from a defective history of man in the state of health and disease, we are unable to assign an adequate physical cause to explain the phenomena; instead of terminating inquiry by feigned or *nominal solutions*, it becomes us to prosecute the subject still farther, with patience and sagacity*."

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* On the blood, page 190. † Ibid, page 200.

From a knowledge of the change produced on the coats of the stomach after death, by the action of the fluid which it secretes, some advantages may have occurred in researches by dissection; but we can scarcely admit this to have been one of the most important "or striking of Mr. Hunter's discoveries;" granting, what is by some denied, that he was the discoverer. We are not amongst those with whom "it became a matter of astonishment that the stomach which digested every thing else should not be digested by its own contents †," because we could never suspect that in the animal fabric, where so much wisdom is displayed, an error so egregious could appear; nor are we much surprised that after death it should so seldom occur. Mr. Hunter indeed says that "it is an appearance we *often* find in dead bodies ‡," and in the next page speaking of the stomach he remarks, "there is *frequently* an aperture made in that viscus;" and he goes still farther than this, for he asserts, that "there are very few dead bodies in which the stomach is not at its great end in some degree digested ||." The frequency of this occurrence, together with the circumstances under which it appears is greatly altered in Dr. Adams's representation of

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* Pearson's Principles of Surgery, sec. iii. p. 19.

† Adams' Preliminary Remarks, p. 29.

‡ Observations on the Animal Economy, p. 185.

|| Ibid, p. 186.

what Mr. Hunter has narrated : “ for this purpose he saw that an animal must be in health immediately before death, otherwise neither the quality nor quantity of the secretion could be equal to the purpose. He was confirmed in this by the instances in which he saw the stomach digested. *Both* were men who had died a violent death ; *both* had been previously in sufficient health to eat a hearty meal. The fair inference from these was that when men die of disease, the appetite usually ceases, and probably the secretion of gantric juice also[*].” Now in opposition to this statement, besides what has been already quoted, we may add, that the first case mentioned by Mr. Hunter was examined at the request of Sir John Pringle, in consequence of death by disease, and that the two instances alluded to by Dr. Adams, are mentioned in a note as “ the first in which Mr. Hunter had occasion to observe this appearance in such as had died suddenly from violence[†] ;” but in several other persons who like these had died from having the skull fractured, and in one who had been hanged, no such change could be detected.

To account for this abridgment of events and circumstances in the statement of Dr. Adams, it is only requisite to know that he wishes to remove an objection to the inference, not derivable from the details of Mr. Hunter, by connecting with the alleged infrequent fact of the digestion of the stomach, a speculation respecting the modifications of death, introduced into an essay on the recovery of persons apparently drowned. The extension of knowledge, produced by thus bringing together two points which had not by Mr. Hunter been placed in contact, being somewhat similar to that of Mr. Gibson, (whom Dr. Adams “ endeavours to keep in view,”) in another instance, the praise so lavishly bestowed by the Doctor, must unavoidably return by reflection to himself. Mr. Hunter had proved by an experiment that the elongation of the cylindrical bones “ did not take place through their whole length, but at the two ends ;” and Mr. Gibson, in a sensible, but by no means extraordinary performance, has shewn that the elongation of the bone is not from its head, but from “ the point of contact with the epiphysis[‡] :” “ here is an oversight of Mr. Hunter corrected.” Mr. Hunter has proved by dissection that the stomach is digested after death ; Dr. Adams has attempted to prove why “ we meet with the case so seldom in the human subject ;” here a subject left unfinished “ is completed by pursuing the inquiry in the manner he has begun;" to each therefore may with *equal* propriety be applied what the *modesty* of Dr. Adams has appropriated solely to Mr. Gibson. " A less ingenuous mind might have triumphed in having seen what was overlooked by so accurate an experimenter, or at least have claimed the merit of completing what Mr. Hunter had left unfinished. Here we see a pleasing instance of the advantages derived from a well regulated mind—from that harmony which pervades every part of the conduct, whilst the object is to gain and to communicate information *."

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[*] Morbid Poisons, Preliminary Remarks, p. 31.

[†] On the Animal Economy, p. 25,

[‡] Memoirs of the Manchester Society, vol. 6.

The omission above alluded to, originated in Mr. Hunter's neglect of his own distinction between " absolute universal death, and such a cessation as can never be restored, of all the actions by which life is supported." Dr. Adams proposes to " assist us by using for these two states the common terms, expiration and death. When a man ceases to breathe, we may truly say he has expired, but we cannot call him dead, nor does a medical man pronounce him so, till his limbs have stiffened, and afterwards have lost their rigidity, or till putrefaction commences *;" or perhaps we may soon have occasion to add, until he has resisted the re-animating powers of Colonel Riddell's miraculous powder †. Of the etymological acuteness of Dr. Adams, this cannot be deemed a flattering specimen, for although on occasions not demanding extreme precision, we may be allowed to say that he who has ceased to breathe has expired, yet as the term expiration has hitherto always been employed in opposition to inspiration, constituting in succession a process confined to animals indisputably alive, the change can lead to nothing but confusion.

The distinction, however, which is here maintained, did not terminate in verbal consequences merely; it induced Dr. Adams to make trial of every mode, by which death might with the greatest celerity be inflicted. Blows on the stomach after the example of Mr. Hunter, were in one instance applied to a dog, and seven hours afterwards, a hole was found in the great end of that viscus; an electrical discharge from a powerful battery was resorted to, but this required three or four shocks, which was a process not sufficiently rapid, though two blows on the stomach were expeditious enough; besides the experiment had to contend against the disadvantages of evening, which in respect to the day, like winter in respect to the year, is most unfavourable for this destructive purpose: dislocation was practised on the neck of a rabbit, which gave the desired effect on the stomach; and wounding the spine of another, which did not. In the several instances in which the stomach presented the wished for appearance, the animal had been killed in such a manner, that neither stiffening of the body nor coagulation of the blood followed, and hence the stomach is supposed to have been destroyed so quickly, that there was no opportunity for the contraction to occur which is stated to take place in the muscular organs of animals, unphysiologically denominated dead. Without being prepared to offer an opinion on this difficult subject, which to be of value ought to proceed from an accurate knowledge of the relation of the nervous, muscular, and fluid parts, we may assert that something more than difference in time alone must be admitted. We are told that a person dying in consequence of a violent ebullition of anger, or by a discharge of an electrical torrent from the clouds, or an animal from motion long continued in running, (not surely the most expeditious kind of death,) are in respect to stiffness and coagulation similarly circumstanced; yet whatever may be the change produced in the first case through the medium of the nerves, in the second we are sure that there has been exerted the most powerful decomposing influence, and in the third, the fixation of a larger than usual quantity of oxygen, which Seguin has proved by experiment is consumed in much greater abundance during even moderate exercise.

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* Preliminary Remarks, p. 27.

† Vide Gentleman's Magazine, for December, 1807.

We must also contend in opposition to Mr. Hunter and Dr. Adams, that the usual rigidity or stiffness of the body after death is not owing to muscular contraction. It is well known that when the last convulsive struggles have ceased, a limb placed in any position, the flexibility still remaining, will continue unmoved, yet in that position it will become firm, even more firm than when in the greatest degree of contraction during life. If the jaw have fallen at the instant of the relaxation of death, unless supported in the elevated position until the muscles commonly used for this purpose become solid, it will not be raised, yet in that state of extension it will acquire the greatest degree of hardness: farther, if the "stiffening" were occasioned by muscular contraction, we ought always to find, (the tenseness being equal or superior to that during full action in life,) an exhibition of the excess of the power of the flexors, and the body before attaining to its ultimate firmness, ought to be bent in compliance with this exertion. The stiffness we believe never occurs where from changes in the chemical qualities of the fluids, they will not consolidate and coagulate by cold and rest, and therefore we consider the coagulation of the blood in the minute muscular ramifications, a cause of inflexibility so far as it is concerned, rather than with Mr. Hunter, an effect of remaining vital power, corresponding with that which may be supposed still to exist in muscular structures. In one instance of very unusual stiffness after death by accident, we have been informed that the synovia became so solid, as to produce a crepitus, on attempting to bend the limbs.

Muscular contraction of this, in many classes of animals slightly muscular organ, having probably no concern in regulating its digestibility, to account for the variations mentioned, we must refer to the state of the gastric fluid and the ingesta, at the time of death. Experiments, such as will afford the assistance we require, have not yet been performed; but we believe they have already furnished some information, by shewing that the stomach of a rabbit, allowed to die for want of food, (a death not to be distinguished for expedition) was digested, whilst in one permitted to satisfy its hunger, and which in a short time after was destroyed, no such appearance was observed. Some assistance might also be derived from attending to the digestive organs of fishes, which, as described by Cuvier and others, are various and dissimilar: in them the stomach is digested, Dr. Adams supposes because they die quickly; we apprehend it is because the stomach is very thin, and the gastric juice peculiarly active. As the food of fishes is swallowed without mastication, the structure of the teeth and jaws not admitting of this process, and without division, even when the prey is large, because, being surrounded by a fluctuating and unsteady element, if left for a moment it might be washed away, a powerful solvent to act on solid substances and quickly remove the mass from the short gullet, has been provided; exhibiting a remarkable instance of compensation, similar to those described by Dr. Paley in his Natural Theology. This interesting subject, we hope, may soon undergo an experimental revision, and it will certainly reward the patient and intelligent investigator. The inquiry ought to be conducted without prepossessions, and therefore in the prejudiced hands of our author we fear it can never prosper.

We have been induced to bestow an extraordinary degree of attention upon the introductory part of this work, on account of the scientific influence of the individual, whose physiological speculations it has been the object of our author to revive and extend. On the pathological portion, considerably the largest, we shall not have occasion to employ a proportionate quantity of criticism, because it includes much useful description of disease detached from all hypothesis. To the pathological opinions of Mr. Hunter we shall make many objections; yet, when taking a general view of his achievements, we shall not be found unwilling to offer our tribute of praise; feeling convinced, however, that we still shall not be prepared by a devout admiration of his talents, to join Dr. Adams in the celebration of his Apotheosis.

Art. VIII. *Remarks on the Reform of the Pharmaceutical Nomenclature, and particularly on that adopted by the Edinburgh College.* By John Bostock, M. D. late President of the Edinburgh Medical Society, Member of the London Medical and Chirurgical Society*, &c. &c. 8vo. pp. 54. London, Longman & Co. 1807.

THE able author of this pamphlet, although himself thoroughly conversant with modern chemistry in all its details, and a strenuous advocate for the revolution which has of late years taken place both in the principles and language of that science, is a decided enemy to the introduction of similar changes in pharmaceutical nomenclature. And he criticises more particularly the last edition of the *Pharmacopeia* of the Edinburgh College of Physicians, in which a change of this kind has been carried into effect.

Dr. Bostock begins by observing that in medicine the chemical composition, and scientific names of remedies, are only secondary objects, their action *on the living body* exhibiting their most essential qualities. Having thus endeavoured to prove that such a reform was not essentially necessary, he proceeds to shew that it is likely to be followed by injurious consequences.

The ground on which this opinion rests is displayed in the paper alluded to with great perspicuity and candour, and the principal arguments when brought into one view, may be comprehended under the following heads:

1st, The danger of insufficient chemical knowledge in a very great proportion of medical practitioners, and the probability of frequent mistakes arising from this circumstance.

2ndly, The confusion occasioned by such a change in medical publications.

3dly, The different degrees in which the various colleges of Great Britain will admit those changes, and the confusion likely to arise from this difference.

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* The Editors received by an indirect communication, this short abstract and criticism of Dr. Bostock's publication which, from a wish that the subject should undergo a thorough discussion, and a high respect for the opinion of the writer, they are happy in the opportunity of presenting to their readers.

4thly, The probability of future improvements taking place in chemistry, and consequently of the necessity of making further alterations in the adopted nomenclature.

5thly, An imperfect knowledge of the precise chemical nature of many pharmaceutical ingredients.

6thly, The long and complex names often introduced to express some of the most familiar pharmaceutical compounds.

7thly, The danger, lest errors in the composition of a substance, should occasion mistakes in its application.

Lastly, The imperfect manner in which the reformation in question has been accomplished by the Edinburgh College of Physicians, and the extreme difficulty of avoiding such imperfections.

With regard to the particular criticisms of Dr. Bostock on the new Edinburgh Pharmacopeia, it cannot be denied that many unnecessary alterations and far-fetched terms, especially in the botanical department, have with some justice received his animadversions. It must be allowed, for instance, that the names *mucilago mimocæ niloticæ*, to express mucilage of gum arabic, or those of *Decoctum Anthemidis nobilis* or *decoctum hordei distichi**, to express camomile tea, or barley water, appear rather pedantic.

It will also readily be granted to Dr. Bostock that the propriety of adopting a systematic nomenclature in chemical language, is a question much more easily decided than that of introducing a similar revolution in practical pharmacy. Yet when we consider whether our present heterogeneous pharmaceutical jargon should be continued, or whether we should adopt other terms more uniformly founded on our received system of chemical knowledge, (however unstable this foundation may be,) we may reasonably question the validity of Dr. Bostock's objections to the alterations alluded to.

One principal reason for so far differing from Dr. Bostock in this respect, is precisely one of those which he has urged in supporting the other side of the question, viz. the difficulty which practitioners of pharmacy, as well as the older physicians would experience, in making themselves familiarly acquainted with this new language. For it may be reasonably assumed that they would soon find that in opposition to this difficulty, mere memory and routine would lend them but a feeble assistance, and in order to diminish the mechanical labour of learning these new terms, and the perpetual danger of committing errors, they would think it worth their while to learn the modern doctrines of chemistry. One or two generations of medical practitioners, would, it is true, feel some inconvenience from the innovation, but the advantage accruing would be permanent. For although it is probable that the progressive improvements in chemical science would impose a necessity of making occasional alterations in the language, yet these would be gradual and easily followed up, whilst on the other hand, the habit of never using words without their being connected with some well defined meaning, would operate as a gradual improvement on all the branches of medical science; which, perhaps, more than any other requires a reform in its mode of philosophizing.

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* Stronger examples of the alleged superfluity might have been adduced, Letter for letter, we believe it would be difficult to establish a preference. Among the following terms mucilago mimosæ niloticæ, *mucilago gummi arabici*, decoctum authemidis nobilis, *decoctum florum chamæmeli*, decoctum hordei distichi, *decoctum hordei perlati.*

As to the danger of mistakes, we conceive that no great apprehensions ought to be entertained. Blind routine is perhaps more dangerous than the temporary embarrassment alluded to; particularly as in the present case every man would be upon his guard, and as the difficulty, after all would be ultimately reduced to that of framing for the use of the older practitioners, a convenient vocabulary of synonimous terms.

An objection is started by Dr. Bostock which applies to Great Britain with particular force, we mean the confusion of languages likely to arise from the different pharmaceutical terms used by the colleges of London and Edinburgh, and by the students of the medical schools of either metropolis. But if the change of nomenclature should be thought desirable upon general grounds, might not this objection be removed by a resolution of the colleges to blend their pharmacopeias, and to establish, by mutual concessions, a common standard of nomenclature? By this liberal system the college of Edinburgh might perhaps in some degree be checked in that bold spirit of innovation, which depends partly upon the keenness with which some of its professors keep pace with all modern discoveries and improvements, and partly upon a contracted system of education, which enables them to diffuse those changes throughout their school with great rapidity; and the college of London would perhaps be stimulated to a little more decision and quickness in its proceedings, which are often checked by that dilatory spirit and excess of caution, too generally attending the deliberations of large and important bodies.

These remarks are thrown out less with a view to offer a correct analysis of Dr. Bostock's pamphlet, which would require a much more minute examination, than to direct the attention of the medical world to a subject of the highest importance, particularly as it at this moment occupies the consideration of the College of Physicians of London.

LIST OF MEDICAL PUBLICATIONS.

Observations on Emphysema; or, the Disease which arises from an infusion of Air into the Cavity of the Thorax, or Subcutaneous Cellular Membrane. By Andrew Halliday, M. D. Member of the Royal Medical and Natural History Societies, Edinburgh, and Fellow of the Royal Highland Society of Scotland 8vo. 5s. boards.

Researches, Anatomical and Practical, concerning Fever, as connected with Inflammation. By Thomas Beddoes, M. D. 8vo. 6s. 6d. boards.

Remarks on the Reform of the Pharmaceutical Nomenclature, and particularly on that adopted by the Edinburgh College. Read before the Liverpool Medical Society. By John Bostock, M. D. &c.

Additional Cases of Gout, in farther proof of the salutary efficacy of the Cooling Treatment of that afflicting Disease, with illustrative annotations, written Authorities in its Support, Controversial Discussions, and a View of the present State and future Prospects of the Practice. By Robert Kinglake, M.D. Member of the Royal Medical Society of Edinburgh, of the Royal Physical Society of Gottingen, &c. &c. In one vol. 8vo. closely printed, price 8s. 6d.

The Physician's Vade Mecum, being a Compendium of Nosology and Therapeutics, for Students. By the Rev. Joseph Townsend. 10th Edition, foolscap 8vo. 4s. boards.

A Dictionary of Chemistry and Mineralogy, with an account of the process employed in many of the most important Chemical Manufactures: to which are added, a Description of Chemical Apparatus, and various useful Tables of Weights and Measures, Chemical Instruments, &c. By A. and C. R. Aikin. In 2 vols. 4to. closely printed, price three guineas and a half in boards, illustrated by fifteen engravings.

Chemical Philosophy, or the established Bases of Modern Chemistry. By A. F. Fourcroy. 3d edition, 8vo. 7s. boards.

A Treatise on Hernia; being the Essay which gained the Prize offered by the Royal College of Surgeons, 1806. By William Lawrence, Member of the Royal College of Surgeons in London, and Demonstrator of Anatomy at St. Bartholomew's Hospital. 8vo. 9s. boards.

A Letter on the Practice of Midwifery, occasioned by, and including an account of an unfortunate Case, with some Observations and Reflections on the subject. By John Boys, Physician, Man-midwife to the Westminster General Dispensatory, and Teacher of Midwifery in London. Price 1s. 6d.

The Philosophy of Medicine; being Medical Extracts on the Preservation of Health and Cure of Diseases; including the Laws of Animal Economy, and the Doctrines of Pneumatic Medicine. By Robert John Thornton, M. D. 5 vols. 8vo. 3l. in boards, 4th edition.

An Essay on the Pathology of the Human Eye. By James Wardrop, Fellow of the Royal College of Surgeons, Edinburgh, &c. In royal 8vo. with Engravings by Meadows, Medland, Madocks, and Heath.

Hunter on Diseases of India Seamen and Lascars on long Voyages, price 15s. sewed.

Elements of Science and Art; being a familiar Introduction to Natural Philosophy and Chemistry. Together with their application to a variety of elegant and useful Arts. By John Imison. A new edition, considerably enlarged, and adapted to the improved State of Science. By Thomas Webster. In two vols. 8vo. with 30 plates.

An Elementary Treatise on Natural Philosophy, from the French of M. R. J. Hauy. By Olinthus Gregory, A. M. 2 vols. 8vo. 24s. boards.

The Chemical Catechism, with very considerable additions. By Samuel Parkes. A new edition, price 12s. in boards.

The modern Practice of Physic, by Edward Goodman Clarke, M. D. of the College of Physicians, London, and Physician to the Forces, &c. The 2d edition, 8vo. 9s boards.

An Attempt at a Systematic Reform of the Modern Practice of Adhesion; on the Use and Abuse of the Thread Suture; with a View of its Merits comparatively with those of the Adhesive Strap, in the Surgery of Wounds. By Samuel Young. 4to. 14s. boards.

The Climate of Great Britain; or, Remarks on the Change it has undergone particularly within the last 50 years; with the Effects such ungenial Seasons have produced on the Vegetable and Animal Economy. By John Williams, Esq. 8vo. 8s. boards.

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The Edinburgh Medical and Surgical Journal; exhibiting a concise View of the latest and most important Discoveries in Medicine, Surgery, and Pharmacy. No. 13. 3s.

Observations on the Diseases incident to Seamen, retired from actual Service, by reason of Accidents, Infirmities, or old Age. By Robert Robertson, M. D. F.R.S. F.A.S. 4 vols. 8vo. 1l. 16s. boards.

A Practical Synopsis of the Materia Alimentaria and Materia Medica; comprising the latest Improvements in the London, Edinburgh and Dublin Pharmacopeias. By Richard Pearson, M.D. Member of the Royal College of Physicians, and formerly Physician to the General Hospital near Birmingham. 8vo. 12s. boards.

Observations on Aneurism, and some Diseases of the Arterial System. By George Freer, Fellow of the Royal College of Physicians, London, and Surgeon to the General Hospital, Birmingham. 4to. 21s.

Treatise on Pulmonary Consumption; in which a New View of the Principles of its Treatment is supported by Original Observations on every Period of the Disease: To which is added, an Inquiry, proving that the Medicinal Properties of the Digitalis, or Fox-glove, are diametrically opposite to what they are believed to be. By James Sanders, M. D. one of the Presidents of the Royal Medical and Royal Physical Societies of Edinburgh.

TO OUR READERS.

Some articles in the present number having exceeded the expected limits, we reluctantly postpone the review of Dr. Trotter's Essay on the Nervous Temperament, and Dr. Hamilton's Essay on Digitalis.

THE

LONDON MEDICAL REVIEW.

MAY, 1808.

Nº II.

Art. I. *A View of the Nervous Temperament; being a practical Enquiry into the increasing Prevalence, Prevention, and Treatment of those Diseases commonly called Nervous, Bilious, Stomach and Liver complaints; Indigestion; Low Spirits; Gout, &c.* By Thomas Trotter, M. D. &c. &c. &c. 8vo. pp. 328. Longman, and Co. 1807.

DR. TROTTER's book is very neatly printed upon wove paper, price seven shillings, and his title page is calculated to excite the envy and admiration of all needy practitioners, for it appears that he has been physician to four squadrons and as many admirals, and is besides a member of three Scotch societies. How boundless is the ambition of man! Not contented with all these honours, Dr. Trotter chooses to enter the lists as an author, and notwithstanding the veneration which we feel for his many titles, we are compelled to review his book as we should the work of a man who had never felt the pulse of an Admiral. In a treatise on nervous diseases, we did not expect to encounter any political discussions, nor till we had perused the present work, were we aware of any necessary connexion between the liberties of Europe and the great sympathetic nerve.

Dr. Trotter may be considered a new alarmist; he tells us there is but one way of preserving our political existence; we must check the increasing prevalence of nervous diseases *, which if not soon restrained will inevitably sap our physical strength of constitution, make us an easy conquest to our invaders, and ultimately convert us into a nation of slaves and ideots.

Nervous disorders, we are told, form two thirds of the whole catalogue of diseases with which all civilized societies are afflicted,

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* Vide Dedication, page xi.

flicted, and in Britain probably a still larger proportion, because so many of their causes are to be traced to its peculiar situation, its insular varieties of climate, and atmosphere, its political institutions and free government, and above every thing its vast wealth so diffused among all ranks of people*.

The first chapter bears this promising title, "The health of the savage state compared with modern times." Here we are informed, "that all the savage *tribes* of the new Continent when compared with European *manners*, are observed to be chaste, temperate, and abstemious." We will say nothing about the phraseology of this *savage* panegyric, nor will we be particular about the truth of the assertion; and though the Aboriginal Americans are in danger of becoming extinct from their excessive use of spirits, we will conclude them to be temperate, and though Europe is indebted to them for the venereal disease, we will give them credit for chastity. Dr. Trotter praises the healthful system of manners practised in the woods of Germany, and quotes Tacitus to prove the chastity of the barbarians who formerly inhabited them. There are some particulars respecting the ancient Germans which Dr. Trotter does not think proper to notice, viz., their custom of getting drunk with mead and their voracious and long continued banquets. No doubt it is true that what are commonly called nervous disorders are less frequent among savages than among civilized nations. It is however a great error to suppose the savage state upon the whole more healthful than the civilized, though the diseased bear no proportion to the number of healthy barbarians, yet it should be recollected, that none but the most robust can exist, and that all whom disease or accident may disable, are left to perish for want of proper assistance. Dr. Trotter's mode of arguing on this subject reminds us of the French surgeons, who abused the English for the number of wooden legs which are seen in this country, when the English returned the compliment by saying, that those who are now walking upon stumps, would if treated by French surgeons, be laid in their coffins; this answer somewhat modified is applicable here. When we hear of the exemption of savages from disease, we should recollect, of the numbers born how few arrive at maturity, and of those afflicted with any disease, how small a proportion recovers.

After commending the ancient Germans and praising the feudal system, Dr. Trotter goes back to the patriarchs, and quotes the 18th chapter of Genesis to shew the simplicity of their diet. An account is given of the entertainment which Abraham and Lot gave the Angels, which the Doctor adds would be considered poor fare by the epicures and gluttons of modern times who dine at the Lord Mayor's feast. The remaining part of the chapter contains an eulogium of the savage state, large farms are reprobated as tending to diminish our bold peasantry, and the disadvantages of refined life are shown to be particularly evident in the lying-in state.

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* Vide Introduction, pp. xvii. and xviii.

The second chapter is entitled "A medical description of the inhabitants of a town or city, being an analysis of society." This analysis contains a list of those who are particularly subject to nervous diseases, viz. Literary men, men of business, the idle and the dissipated, the artificer and manufacturer, persons returned from the colonies, the female sex, consisting of the higher, middling, and lower orders of women.

We have next an enumeration of the remote causes of nervous diseases.

"The remote causes of nervous diseases are chiefly to be sought in populous towns; and increase in proportion to the deviation from simplicity of living. The state of civilized mankind is thus exposed to causes of bad health, which have no power among rude nations, viz. in

"1. Air.—2. Exercise.—3. Food.—4. Clothing.—5. Passions of the mind.—6. Intense study.—7. Lactation.—8. Miscarriages and premature labours, &c.—9. Climate.—10. Medicine."

What a formidable catalogue of causes! and when we consider the foregoing list, it is astonishing that any being who has the misfortune to be a member of civilized society, should be exempt from this universal calamity. Under the head medicine, we have a list of all those drugs which in the hands of any practitioner not particularly attentive to the nervous temperament, are highly detrimental. Dangers indeed threaten us on every side; if our parents were affected with any of the numerous diseases which are denominated nervous, we inherit their distemper; and should we happily derive our origin from the few who are by great good fortune exempt, yet are we liable to acquire this ill-fated temperament by a thousand causes. If our habits are literary we shall become nervous in our closets, if men of business in the counting-house; should we by ill fortune be born in the higher ranks of society, luxury will attack our nerves; and if poor we may be engaged in some manufacture and compelled to live in a town, and then there is no escape from this all-seizing calamity. Live where we will, and in what line of life soever we may be placed, we are liable to some disease, and then we may be obliged to employ medicines which will induce the nervous temperament, and this temperament our posterity must inherit.

The fourth chapter of this medico-political work, treats of the influence of nervous diseases on the character of nations, and on domestic happiness. As this is a subject of general interest we shall allow Dr. Trotter to speak for himself, fearing we might not do justice to his arguments if we attempted to compress them.

"As we analyze the manners of society, and scrutinise those causes which lead to the diseases of which we treat, we observe their operation and influence to be very general, and daily increasing. Commercial Britain, enriched by manufacture and colonial wealth, when compared with her barbarous state, does not exhibit a nobler spirit of independence, or show more fortitude in opposing French invasion, than what was done nineteen hundred years ago by our forefathers, in repelling the Roman legions. Our invaders were then obliged to build strong walls and ramparts to shelter them from the Britons: but modern Britons have erected Martello towers, projected the inundation of Essex, and hoarded up the current gold coin, as tokens of being afraid of the French. These alarms are to be considered as so many symptoms of a nervous temperament appearing in our national character. Britain had no ships to oppose the landing of Julius Cæsar; but at this moment she has a navy capable of fighting the whole fleets of Europe united; yet she trembles at a flotilla of cock-boats. It is that puddle of corruption, the Stock Exchange; that Delphi of Plutus, where stock-brokers pay their vows, and expound prophecies, that has filled the nation with degenerate fears, apprehension, and hypochondriacism. Europe has been saved by the British navy from the fangs of French tyranny; yet shameful to be told, after the fleets of France have been annihilated, we are now to be assailed by three thousand wherries, manned with *blue devils!*

"If I am altogether correct in saying, the *nervous temperament* is hereditary, and therefore the diseases which depend upon it, liable to be extended to the offspring, they must multiply in prodigious proportion. They appear in the present age to have acquired that growth, which nothing but a general revolution in all ranks of society can check. I may be told, that these diseases are not very dangerous, and very seldom produce death; and it may be added, the bills of mortality do not justify my conclusions. These arguments are easily answered. It is true, death is seldom put down to nervous disorders: but if constant pain, mental disquietude, and apprehension of dying, are to be considered as evils in this stage of existence, then are nervous afflictions to be held as the chief cause of them. And it is to be remembered, that the most frightful part of the catalogue of diseases, such as apoplexy, palsy, madness, melancholy, epilepsy, convulsions, cholic, iliac passion, atrophy and dropsy, are often ushered in by nervous affection, before they assume their own character and shape.

Physical strength of body cannot be long preserved under enervating modes of living: the stature and the mind must both diminish and degenerate. The poet says, *fortes creantur fortibus et bonis;* and very aptly adds,

> ——— nec imbellem feroces,
> Progenerant aquilæ columbam. Hor.

"An eminent senator, Sir John Sinclair, has lately made some noble attempts to regenerate the physical strengthof the country, by recalling mankind to agricultural life. His institution of a society for that purpose under the authority of parliament, has already had considerable effect. Many noblemen and gentlemen, of great landed property, have seconded these patriotic views, and turned their attention to this national object: a spirit of improvement has appeared in every county, and is daily increasing. No man who possesses the smallest spark of love for his country, but must wish full success to the undertaking. No truth in political economy is better proved, than that a nation of sedentary people, can never be a nation of heroes. We might indeed preserve all our manufacturing establishments, and most of the fine arts, during the degeneracy of physical courage: but could an army of man-milliners defend the British islands against the ruffians of Bonaparte? The puny inhabitants of Italy and Indostan, are finer spinners of silk and cotton than Englishmen, because they are strangers to all the masculine virtues. Let us therefore beware how we extol too much the effeminate labours of the spindle and the loom. A commercial people merely, can never be an independent nation. They owe to foreigners the consumption of their manufactures; and when these chuse to do without them, or to buy them elsewhere, such a people must become bankrupts in finance. The glory and security of the United Kingdom, must be to blend these pursuits, agriculture and commerce, in the national character."

We trust it is not yet too late to save the country. Dr. Trotter has not only pointed out the disease, but he has furnished some valuable hints towards the cure. Let us destroy that puddle of corruption, that Delphi of Plutus, the Stock Exchange. The three thousand wherries manned with blue devils we do not exactly understand; but we presume the Doctor will himself encounter them, or leave them to the care of Joanna Southcott. Having disposed of the Stock Exchange, we must put an end to the effeminate labours of the spindle and loom, and having annihilated all man-milliners, we may have some chance of checking the formidable progress of the nervous temperament and saving the nation. A methodical history of nervous diseases Dr. Trotter says, is impossible; the only thing certain and peculiar in their character is predisposition hereditary and acquired.

Hereditary predisposition may be discovered very early in life. When children are affected with bilious and spasmodic affections, when their bowels are easily deranged, we are to conclude that these are of the nervous temperament. Should bad bread, heavy pudding, fruit, sweatmeats, pye-crust, pastry or pound-cake disagree with a child, it is all put to the account of the nervous temperament*. In after-life, gout, gravel, jaundice, and almost half the diseases to which men are subject, enable us to judge of the presence of this temperament. Nervous and bilious complaints are used as synonimous terms †. The enlargement of the liver in icterus or hepatitis is owing to the debility of the hepatic nerves. All these (nervous) diseases Dr. Trotter observes receive a strong tincture from the manners of the age; but he adds that none of those persons denominated Quakers are severely troubled with them. All those who are conversant with Quakers however, have remarked that the females of that sect are peculiarly liable to dyspepsia and hypochondria, but these trifling contradictions are not worth noticing. Savages we are again told are not subject to nervous diseases, yet it is a well known fact that savages are far more liable to tetanic diseases than men in any other state of society. We will extract as much as our limits will allow, lest we should be thought unjustly severe upon Dr. Trotter.

"We know so little of the nature of the nervous power, that we can only judge of the moral causes of nervous indisposition, from their effects. Some of the passions have received the name of *exciting*, and others that of *depressing;* but their effects on the *nervous temperament*, seem much alike. The chief of the passions, such as anger, joy, grief, fear, &c. destroy appetite, disturb digestion, prevent sleep, make the breast labour, and the heart palpitate; render the mind fickle, timid, incapable of judging accurately, &c. Here is no proof that any of these passions weaken the nervous system; they only dissever that combination, or association of ideas, which impresses the mind with pleasing objects, and which disseverment to the mind is painful, and throws all the more sensible organs of the body into immediate disorder. The mind and body being connected by the *nervous system*, the same train of symptoms appear, when those causes are applied to the body, which first affect the chilopoietic viscera; such as a mercurial course, or a debauch with ardent spirit or opium. Now, what can be the reason, that these passions, or the articles just mentioned, have such extraordinary action on nervous people, while on others they have no such effect? I would explain the fact in this manner: the hereditary temperament is supposed to inherit all the bad impressions of its progenitor, hoarded as it were in the structure of its nerves, in like manner the acquired temperament retains, or records, as it may be termed, all the effects of vicious indulgence. So that when any fresh gust of passion arises, or any luxurious stimulus is applied to any portion of the sympathetic nerve, these accumulate the quantum of predisposition; and a *nervous fit*, or a *bilious attack*, is the immediate consequence of every new trouble of mind, and of every recent debauch of the body. Thus the habit may become so completely nervous, or in other words, the *predisposition* may arrive at that height, that the faculties of the soul will be worn out, and fatuity take place; and the body will be so enervated as to be in a state of constant pain, tremor or convulsion. Such cases, in no small numbers, are certainly to be seen every day, by medical observers who possess discernment to appreciate experience."

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* Vide page 226. † Vide page 118.

At the conclusion of the chapter on the general doctrine of nervous diseases, Dr. Trotter asserts that their pathology is to be sought in the deranged sensations and inverted sympathies of the great sympathetic nerve*!

In the last chapter which treats of the prevention and cure, we are told that very novel truth, that man is the creature of habit. Hence it is inferred, that milk is the proper food for infants, and that if a mother cannot suckle her own child, cow's milk may be substituted. It is however very important that the cow's diet be attended to, and that grains and turnips should be avoided; when two years old the child may be allowed fish and eggs, a little broth and no pastry; puddings made of rice are tolerated. With regard to the cure, exercise and moderate diet are chiefly insisted on. Let us take this opportunity of praising Dr. Trotter for the strong manner in which he states the importance of attention to diet, and a rigid adherence to temperance, in all those who have any tendency to nervous disease. This advice is applicable not only to those who are of the nervous-temperament, but to men of all temperaments and of all ages. Dr. Trotter's advice on the subject of diet we can praise with confidence, but to what class of readers are we to recommend the present work? Here we are again puzzled: certainly we cannot conscientiously advise any medical man to purchase the View of the Nervous Temperament, for he will find nothing that is not contained in Dr. Whytt's works, nothing but what he must have known at the very commencement of his medical studies. It is probably therefore to general readers that Dr. Trotter would wish us to recommend his book. There are doubtless a number of middle-aged ladies in every part of the kingdom, who are troubled with flatulence and the fear of invasion, and who will be delighted to read at the same time about Buonaparte, the Stock Exchange, and the great sympathetic nerve; we cannot do Dr. Trotter a greater service, than by recommending his work to the above-mentioned class of readers.

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* This is a tolerable specimen of the Doctor's modesty, when we recollect that he set out with declaring that there would be found little of what is called theory in the discussion. It is somewhat surprising that Dr. Trotter should have adopted exactly the same theory as Whytt, without appearing aware of the coincidence.

Art. II. *A Treatise on Hernia: being the Essay which gained the prize offered by the College of Surgeons, in the year* 1806. By William Lawrence, Member of that College; and Demonstrator of Anatomy at St. Bartholomew's Hospital. 8vo. pp. 314. London, Callow, 1807.

THE information contained in the title page of this volume, while it conveys a very sensible impression in its favour, accounts for the publication at this juncture of a Treatise on Hernia, and in this view is not without its use. A disease, the history and treatment of which have fixed the attention of the most eminent Continental and English surgeons, and of which the anatomical relations have been lately developed with a copiousness and minuteness unprecedented in the annals of surgical pathology, can receive little light from the labours of the young and the inexperienced. The present author has therefore wisely chosen that department of practical labour which least requires qualification from experience, and which those who possess this qualification in any eminent degree, are generally most averse to fill. A compilation of established facts and authorised opinions, is occasionally of essential service to the progress of science. It clears away the rubbish of innovation, and sets before us in a comprehensive but defined view, the extent of our researches, and our knowledge. It is a gratifying tribute to original writers, giving notoriety to their merits, and influence to their example. It is especially valuable, as it exhibits to the student all that is really useful within the compass of a volume not exceeding one tenth of the size or price of those from which it was gleaned. The requisites indispensable for such a task are assiduity of research, soundness of judgment, and perspicuity of style; requisites which are evinced in the work before us in no ordinary degree.

Much as we should now feel discouraged from undertaking a systematic work on the subject of this Essay, the time has been, and that time *since the days of Mr. Pott,* when this feeling would have influenced us less forcibly than at present. "The treatise of Mr. Pott (says Mr. Lawrence,) is so particularly distinguished by that lucid arrangement, accurate description, and elegant style, which adorn all his writings, that a person who should take up the subject after him, besides losing his labour, would expose himself to the risk of a degrading comparison." Much too as we defer to the opinion of Mr. Lawrence, the labours of Mr. Pott in particular would not so "fright us from our propriety." They are far from possessing that closeness of thought and profundity of investigation, which are to anticipate every inquiry, and satisfy every doubt. It would be invidious to institute a particular scrutiny into the justness of the terms of this high-flown panegyric, but fantastic decorations of thought or language harmonize very ill, we think, with the nature of a scientific subject. In fact, all attempts at a display of poetical taste in writings of this class, are in our opinion, infallible marks of a very bad taste. Lucid arrangement is always an estimable quality, but we hope not a very extraordinary one among practical writers. A phraseology so commanding by force and fluency as that of Mr. Pott, must have proved in some cases a strong temptation to the writer's accuracy; but this could only obtain unwarily and in the nicer shades of verbal description, as his moral principle had not to contend with any vigour of imagination. We have often heard the style of Mr. Pott, characterised by the term "*elegant.*" It is pedantic and finical to excess, always artificial, and yet wanting the art to conceal it. The habit of flowery amplification and bombastic energy of diction is totally at variance with the natural simplicity and terseness of expression, which bespeak a full and philosophic mind. Mr. Lawrence's is a far chaster model. But we consider the remark as altogether erroneous, that the most lucid arrangement, accurate description, or elegant style can supersede the utility of any system, which has personal observation for its basis. By a knowledge of the characters of men, we decide the credit due to their writings, and it is only by a comparison of these writings with each other, that we arrive at the permanent value of the facts they contain. In this view, the statements of Mr. Pott have attained and may yet attain confidence and celebrity, proportioned to their consistency with those of succeeding writers, who derive their facts from nature and their ornaments from truth.

If we have discovered severity upon this subject, it has been excited by the frequent and indiscriminate use of hyperboles of this sort. We would by no means detract from the merits of Mr. Pott as a writer respectable for information and talent, and one who has contributed his fair proportion to the improvement of surgical knowledge; but we cannot, nay, will not bear to be told, that his excellences were such as to extinguish the claims of his predecessors, or to degrade the efforts of posterity. Were we for the sake of example, to attempt a parallel between him and his great contemporary, how would the pompous nothings of Mr. Percival Pott dwindle before the profoundly argumentative discussions of John Hunter! To the former we are indebted for some gentlemanly reformations in points of practice, for some useful hints on injuries of the head, and on ruptures, and not to dwell on the *means for obtaining a radical cure of the hydrocele*, a *lucid*, *accurate*, and *elegant* description of the fistulas of the eye and the anus!! To the latter we owe much of the controul we possess over the ravages of the most inveterate and destructive of morbid poisons, and our acquaintance with those inherent principles of the animal economy, upon which surgery has been erected into a science. Mr. Hunter will be celebrated as the founder of scientific surgery, when Mr. Pott is only known by his bistoury.

In the volume before us, the author has confined his observations to the inguinal, crural, umbilical and congenital ruptures: in turning over its pages we shall select the points of interest which it presents, and briefly subjoin our opinion of its execution and tendency.

Mr. Lawrence, following the division of Richter and Monro, describes the acute or rapid, and the chronic or slow kind of inflammation. This is the most simple and characteristic division hitherto adopted; and if the term of the species pointed to the cause of the complaint, as to local injury for example, in the first, and predisposition in the second case, we should regard it in a practical view as unexceptionable. The spasmodic strangulation of Richter Mr. L. very properly observes, has not one distinguishing feature, and to one or other of the species above-mentioned, all cases of strangulation are referable which call for the interference of art. The practical utility of these distinctions promises to be considerably augmented, by the strength and clearness with which the pathognomonic symptoms of each species have been delineated, by several modern writers. The small, tense, and acutely painful swelling which appears after an inordinate exertion, in the groin of a robust young man, will if disregarded, terminate his life in a few hours. The large old rupture which the patient cannot replace as formerly, and which is growing heavy and painful, may yet remain so for as many days, and produce only the fever of constipation. It is true, these are rare because extreme cases, but they are sufficiently frequent to render the distinction essentially useful as applied to themselves, and sufficiently antityped to enable the sagacious practitioner to trace the operation of one or other of them, upon all the modifications and degrees of stricture.

An operation for strangulated hernia in an infant is uncommon; our author mentions one performed upon a child of fourteen months. The scrotal rupture of children is not always congenital as Mr. Pott supposed, and this case is one of the exceptions.

The instrument maker to Bartholomew's Hospital has constructed a truss, the strap of which is to pass just below the crista ilii, instead of midway between it and the trochanter femoris, with a view to remedy more effectually the inconvenience of displacement from the motions of the thigh. The swelling of the great gluteus in the rotation of the thigh inwards, will be somewhat less at this part, but the proposal has not yet obtained the sanction of experience. The essential improvement of modern surgeons in the treatment of reducible hernia, lies in the application rather than in the construction of trusses, and for this we are wholly indebted to anatomical research. It is this which has illustrated the proper point of bearing for the pad of the truss, and exposed the incorrectness of the old mode of application; which has accounted for its little success in the prevention of fresh descents, and pointed out its tendency to produce septa, and thus effect only a partial cure. For such information we are indebted to systematic writers adventurous enough to succeed Mr. Pott, who more sagaciously than happily observed, that if a truss "*be not so put on as to do good, it will do harm.*"

The following manœuvre Mr. Lawrence recommends in cases where the strangulation seems to have been caused by an accumulation of fœcal matter. "Let the surgeon embrace the neck of the swelling close to the tendon, with the finger and thumb of one hand, and carry them downwards with a moderate pressure so as to remove the contents from the portion next to the ring; this will reduce the size of that part, which he may then attempt to pass into the ring with the other hand."

In the justness of the following criticism we perfectly concur:

"If the efforts at reduction, managed according to the above directions are not attended with success, the following method has been recommended. A strong man, placed in a convenient position near the edge of the bed, supports the lower extremities on his shoulders, so that the patient's head and chest only rest on the bed. Attempts at reduction in this posture, are said to have succeeded after every thing else had failed, and have therefore been highly recommended by some surgeons. I cannot fairly appreciate the merit of this proposal, as I have never adopted the practice, nor seen it employed by others. It does not seem to me to promise any advantages that could compensate for the unpleasantness, trouble, and inconvenience inseparably connected with its employment. The proposer of this manœuvre must have expected to accomplish reduction by the mechanical effect which the weight and dragging of the viscera in the abdomen would have on the protruded parts That this idea is completely absurd, must be immediately perceived by any one who forms a just notion of the natural state of parts, who is aware that the abdomen is accurately full, and that all its contents are preserved in their relative positions by the pressure of the respiratory muscles; that they cannot therefore fall from one part of the cavity to another, but are probably just in the same place, whether the head or the heels be the most elevated point of the body. Reduction is opposed by the pressure which the protruded parts experience, and this position can neither overcome nor diminish that obstacle."

Mr. Lawrence agrees with Richter, Hey, and Desault, in the impolicy of confiding much to the taxis. In his sections on the remedies employed for the reduction of hernia, he collates the great contending authorities. For the practice of venesection for example, we have Pott, Richter, and Callisen; against it, Wilmer, Alanson, Hey, and Cooper. The two latter gentlemen rather incline to the mean, than divide with either party; and Mr. Lawrence prefers with Mr. Hey, to keep a middle course between them. In the acute strangulation of young and plethoric persons blood-letting is assuredly indicated; and if the blood be drawn from a large orifice, while the patient remains in the warm bath, their combined effect will frequently prove efficacious. On the use of the tobacco enema our author copies the opinion and almost the words of Mr. Hey. "The tobacco like every other means, has often failed; but no other remedy has been so frequently successful." The general observations on the comparative value of remedies, and the expediency of operating early, are all of them very good, but none of them very novel.

The anatomy of inguinal hernia is a very accurate piece of description, and conveyed in terms equally luminous and precise. It had occurred to us before we had the pleasure of reading Mr. Lawrence's opinion, that the cases of stricture at the neck of the sac, of Arnaud, Le Dran, and Wilmer, were in fact cases of strangulation at the upper opening; but this conjecture we may remind him, could never have been made, if no one had dared to refuse implicit confidence to the *accurate description* of Mr. Pott *. Mr. Lawrence has the credit of having put upon record the first case of ventro-inguinal hernia in the female.

One caution appears to us to have been overlooked in the directions for exposing and opening the herniary sac, viz. to be aware that the sac of the hernia bears only a small proportion to its superficial and cremaster coats; that the former contains only the viscera, the latter the testicle and viscera. If a bold incision be carried below the fundus of the sac, which in herniæ of any duration adheres inseparably to the tunica vaginalis, the testicle will most probably be injured. The incision for dividing the stricture, Messrs. Sharp and Pott have recommended to be carried *upwards and outwards*; Richter and Bertrandi, *upwards and inwards*; both of which are exceedingly faulty from the variety of relations which the sac bears to the epigastric artery. Hence Chopart and Desault adapt the direction of their incision to this variation, cutting *outwards* in the common, and *inwards* in the uncommon species; a course it must be confessed, full of doubt and danger from the difficulty even in recent cases of discriminating between the species, and the impossibility of such a discrimination where the sac of the common inguinal hernia has attained magnitude from the duration of the disease. The practice of Rougemont to cut *directly upwards*, in cases where this difficulty of decision occurred, has been applied indiscriminately to all cases by Cooper, and is the only method which meets all difficulties and is secure under every variety.

A case related to shew that the lower opening sometimes requires to be dilated, merely to enable the operator to relieve the stricture caused by the upper, furnishes some observations on "wounds of the epigastric artery." "The latter incision which had included the inferior margin of the obliquus internus and transversalis, had completely divided this artery at three quarters of an inch from its origin. It did not appear that the smallest quantity of blood had escaped from the divided vessel." This case, remarkable also for the absence of the symptoms usually characterising mortification, "demonstrates the danger of cutting upwards and inwards, and proves that the epigastric artery may be divided without the smallest hemorrhage ensuing from the division." "I have not been able (says our author) to meet with any recorded cases, in which actual examination has proved a wound of this vessel to be the cause of death." Though the fact here stated is curious from its novelty, we do not upon consideration, regard it with any great degree of surprise. Had the artery been simply opened, it must have bled, and the bleeding would have been proportioned to the size of the orifice; but daily experience teaches that the *division* of a vessel of equal diameter, is uniformly attended with the opposite result. The wound of a branch will give rise to a troublesome efflux of blood, as is proved by a subsequent case in the volume before us, whereas the complete transverse section of the trunk is made with impunity. We have known in the case of arteriotomy at the temple, repeated hemorrhagies from the incomplete section of the vessel. The situation of an artery in soft parts, is moreover particularly favourable to its retraction, and consequently to the formation of that external coagulum, which is the first complete barrier to the effusion of blood *. A practical inference from these facts would direct us to prefer the complete division of the vessel, in all cases of hemorrhage from the third order of arteries, to the use of the needle and ligature.

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* Vide works, 1783. vol. 2. page 18.

The proposal of Mr. Cooper to perform the incision of the stricture without including the sac, Mr. Lawrence thinks liable to many objections. First, to that of difficulty from the close connexion of the sac and surrounding parts. Adhesions by bands of lymph are easily overcome, so that this objection is of no force in recent cases. But in an irreducible scrotal hernia of such permanency as to obliterate the canal of the chord, we have succeeded readily in the attempt to pass a director between the cremaster coat and peritoneum. We confess however, that some manœuvre is generally required for the separation of adherent membranous laminæ; and although the anatomist seldom meets with a hernia of which he cannot distinctly demonstrate the coverings, the operation in unpractised hands would at all events, be objectionably tedious. A second objection to the practice is taken from the diminished chance it affords of a radical cure, since a bag of peritoneum an inch in length, remains open below the ring, and ready to receive a future protrusion. Our author prefers the practice of Richter to scarify the neck of the sac, and thus produce adhesion of its sides. We really feel at a loss to account for the successful result of such a practice. Is it to be credited that the dilatable peritoneum should of itself, oppose a resistance to the re-descent of the bowel, sufficient to establish a radical cure? The proceedings to promote this desirable object by obliterating the mouth of the sac, always appeared to us nugatory in practice and unscientific in principle.

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* Jones on Hemorrhage, p. 54.

In operating on large herniæ Mr. Lawrence judiciously follows the practice of Petit and Cooper.

In the following note of reference to Dr. Monro's work on the Bursæ mucosæ, he has anticipated us in a critical animadversion, which otherwise we should certainly have felt it our duty to make. It is one which has been long called for, and to which no man who entertains a proper respect for himself and his profession, can possibly refuse his assent.

"I cannot leave this part of the subject without performing what seems to me to be an act of justice to the memory of a most excellent surgeon. The honour of proposing the above-mentioned mode of operating, belongs exclusively to JEAN LOUIS PETIT, who recommends it particularly in those cases, to which it certainly is the most applicable, namely, large and adherent herniæ. Dr. Monro (in his *description* of *all the bursæ mucosæ)* recommends Petit's method, particularly because the intestines are not exposed to the air; in which he seems to conceive that some very poisonous and noxious qualities reside; but he adds, that the proposer of this operation did not understand the reasons on which its utility depends. The reader will be much surprised to find, after this assertion, that the French surgeon mentions it as a great advantage of his method, that the protruded parts are not exposed to the air. (Traité des Mal. Chir. tom. II. p. 373.) I do not mention this from attaching any importance to the notion of the deleterious effects of the air, which on the contrary, seem to me to be quite imaginary; but merely to prove that those who are the most zealous in defending their own claims to originality, are not always the most scrupulous in respecting the property of their neighbours. Dr. Monro supports his assertion concerning Petit's ignorance of the true principles on which the utility of his operation is founded, by a quotation, which the reader must have perceived to have no connexion with the subject; and he will accordingly find that the passage in question is taken from a section of Petit's work, in which he is speaking on a point altogether different."

Two cases alone, as it appears to us, admit of the operation of Petit—First, where the rupture is very recent, and secondly, where it is very large. In both we have witnessed its complete success. The objections to it are taken from the very cases in which it ought never to be performed, and thus it has proved an ample field for controversy.

In his chapter on the treatment of the omentum, our author following Sharp, Pott, and Cooper, deprecates the use of the ligature, and takes occasion to amplify a remark of the latter upon the absurdity of removing one stricture to impose another. He supports his opinion by a well drawn case, and objects we think, with as good reason, to the practice of leaving the omentum to granulate in the wound.

In discussing the treatment of intestine with gangrenous spots, Mr. Lawrence advocates the practice of the celebrated Desault, whose experience convinced him that the inflammation preceding gangrene fixes the gut to the ring, and that no effusion of the contents of the canal into the abdominal cavity, takes place on the separation of the slough. De le Faye and Le Dran decidedly state the fact of adhesion of the intestine, and discharge of fœces through the wound; and cases are furnished by Petit, Sharpe, and Cooper, corroborating the testimony of these writers in the latter point. In addition to these the author brings a strong case from the practice of Mr. Long, in Bartholomew's Hospital, where the patient in three weeks from the operation had motions per anum.

"The contents of the rupture were found to consist of what has been termed a knuckle of small intestine. Mr. Long observed when he opened the sac, that the contained fluid had a fœcal smell. The ring which formed a very close stricture had made a manifest impression on the gut, and a small pin hole appeared in this part, through which the alimentary matter came. A broad patch of the posterior part of the intestine was manifestly gangrenous; and a smaller portion of the convexity of the fold appeared in the same condition; the gut was returned *, and evacuations were procured per anum by means of clysters and purgatives. In three days the contents of the bowels began to be partly discharged through the wound, and in a short time they all came that way. The evacuated matter was a light yellow frothy fluid, mixed with flakes of a more consistent kind, it had no fœcal smell, and was discharged in less than ten minutes after drinking. The general health was perfectly good. In three weeks he began again to have motions per anum, which increased in quantity while the discharge by the wound was diminished, and this consisted at last of a mere froth; in a very short time the wound had completely cicatrized, and the man was discharged perfectly well."

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* I have been informed by Mr. Cooper, that in a case operated on at Guy's Hospital, where a small opening was formed in the intestine, the aperture was tied with a fine ligature, previously to its being returned; and that the patient recovered.

The following is the experience of Desault.

"In operating on an hernia, he found an eschar of an inch in diameter on the intestine; he returned this part, and no subsequent symptoms occurred to denote the separation of the slough. He conceives that the inflammation of the part surrounding the eschar, agglutinated it to the parieties of the abdomen; and that the slough passed along the intestinal canal. But it is not on the event of a single case that he rests the propriety of this practice; he recommends it from the favourable result of his general experience. He has relinquished the loop of thread through the mesentery; *being convinced by experience and particularly from dissection, that the portion which forms the hernia never recedes from the ring, and that there is no reason to apprehend an effusion into the abdominal cavity on the separation of the eschar.*"

Where mortification has extended to the whole circumference of the bowel[*], we are also recommended to dilate the stricture, and leave to nature the casting off of sloughs, the apposition and adhesion of the ends of the gut, the closure of the wound, and the re-establishment of the canal. The arguments for such a practice rest upon the result of numberless cases of unquestionable authority, and particularly upon the combined experience of Petit and Riehter. We doubt if the directions of La Peyronie to approximate the extremities of the gut after removing the dead portion, by a ligature of the mesentery; and of Littre and Louis, to tie the lower orifice and so confirm at once the artificial anus, are worthy of any consideration. Does the case of inadherent gangrenous intestine ever occur? or is it ever a justifiable practice to compromise for no adequate object the only chance of a desirable termination?

In this very important deliberation, the authorities upon which we most satisfactorily rely are unhappily at variance. Mr. Cooper mentions two cases in which the mortified part consisted of jejunum and ileum, and both patients fell victims to inanition from the speedy discharge of the food. In another case a small opening was left for the egress of fœces, and an artificial anus was the consequence. These observations have induced him to excise the mortified part, to apply sutures upon the cut ends of the sound intestine; and as ulceration is a rapid process in inflamed parts, he confines it by a ligature through the mesentery, to prevent the escape of fœces into the abdomen. We must remember however, that for one case in which the opening degenerates into an artificial anus, we have numerous cases of its permanent closure; and if unfortunately the jejunum or ileum be unsound, sutures are of no efficacy in preventing the discharge of the food[*]. It would be idle to argue from the result of cases which have proved fatal in a few hours from the operation, upon the comparative merits of the practice of Desault and Cooper. The patient is in the hands of death, when he is consigned to those of the surgeon. But it is the imperious duty of the latter to act in all cases as under the impression of the most favourable expectations, although his prognosis will unavoidably be, influenced by the state and progress of the gangrene, and the constitutional strength of the patient.

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[*] Our author very properly rejects without hesitation the botching contrivances of the older surgeons, to substitute a bowel of card or isinglass. We are seriously concerned to find the senior professor of anatomy in the University of Edinburgh, engaged in a controversy so late as the year 1807, to defend his claim *to the honour of reviving the operation of Rambdorius!* Probably Ramhdor like Petit, *did not understand the reasons on which the utility of his practice depends.* The reviver grounds his claim upon success fully equal to that of the inventor; the slight difference in the cases is, that between a *man* and a *pig!!!*

The question to be decided is first, whether in the gangrenous spot, we shall fix the intestine by a ligature of the mesentery opposite the wound? Secondly, whether in the mortification of the whole cylinder, we shall remove the affected portion and bring the cut ends into contact by sutures, or yield the entire management of the case to nature[†]? "We should not, says Mr. Lawrence, be justified in expecting the fœces to spread over the abdomen, even if the intestine were not exactly against the ring. Petit, in his excellent memoirs on effusions[‡], has long ago refuted the commonly received notions on this subject, both by facts and reasoning: he has clearly shewn, that the contents of the intestine or blood shed into the abdomen do not spread loosely over the cavity; that the pressure of the respiratory muscles affords the obstacle to such an expansion; that the effused matters being evacuated in opposition to considerable resistance, are collected into one spot, to which they become confined by the inflammatory agglutination of the contiguous parts, and where they form what the French call a depot." These very ingenious inferences of Petit, admit of some exceptions. If, for example, a healthy bowel be wounded, or one that is in a state of gangrene be completely separated from its adhesions, and lacerated on its return into the belly, nature is taken as it were by surprise, and we must not expect to find that the natural obstacles to expansion, or the natural consequences of inflammation will operate to prevent it. Our recollection furnishes us with two cases in proof of this remark, in which the entire superficies of the viscera was covered with fœcal matter, and the peritoneum appeared to have been in a state of active inflammation. The patients survived the accident for some hours, and experienced during this time the most excruciating torture.

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[*] Vid. Case of Dollet. Coop. P. 2. page 31.

[†] As the utmost difference prevails between the ulceration from a ligature and the separation of an eschar, we should think it advisable in all cases to confine the *sutured* gut to the wound. Mr. Cooper found that an artificial anus was formed in three days from the ulceration by the ligature.

[‡] Sur les Epanchemens de la bas Ventre. Acad. de Chir. tom. 1 & 2.

Where nature employs her own process of separation, a provision is made for the fœcal evacuation externally. It consists in the accurate relation in point of time, which the ultimate bears to the primary termination of inflammation. Where on the other hand, art interferes to produce a solution of continuity in a sound intestine, or to tear through the adhesions of one approaching to the state of gangrene, she must employ her own means to prevent the consequences: hence we are left to conclude that it is only in cases of mortified and *displaced* intestine, that a ligature is required to connect the bowel to the wound; and this is in most instances only an apology for the surgeon's rudeness, in drawing forth the intestine from its seat in the belly, and thus tearing through the ligature of nature. Suppose however, an insulated spot to present itself upon the inferior part of an extensive convolution, either the gut must be returned, or the whole fold allowed to slough. In the first case, the adhesions will of necessity be destroyed; in the second, an artificial anus will be an almost unavoidable consequence. Hence we should not hesitate to replace the intestine in the belly; but having so far interfered in the process of nature, should in consistency with the principle before explained, prevent its departure from the wound by a ligature of the mesentery.

On the second part of the question, we have authority to oppose to authority, and fact to fact. Mr. Cooper states in his last volume, that two cases have occurred to him since writing the former, in which the mortified intestine was left to nature, and both patients died within one and twenty hours. But as we have before observed, these cases do not reach the question, because the effect of neither practice could have been experienced in so short a period. "Almost all the numerous instances of recovery from mortified hernia (says Mr. Lawrence) which are recorded in the annals of surgery, took place when the surgeon was contented to remain a quiet spectator of the process." These instances go a great way to decide the point at issue. It remains for the candid observation of those who have the opportunity, to determine, "*Whether in cases where the patient survives the operation, the removal of gangrenous intestine by the scissars is, or is not an objectionable practice,* *and whether it is giving a fairer chance of recovery, to second the efforts of the constitution during the process of separation, while we facilitate by a free opening the evacuation of the alimentary matters.*" In doing this we do not disturb its adhesions, the indication which of all we are most solicitous to follow, since upon it depends not only the present easy discharge of fœces through the wound, but the prospect of its being ultimately discontinued by the restoration of the canal.

The "anatomy of femoral hernia," is entitled to all the encomium bestowed upon the description of the inguinal. The results of our investigation of this subject are already before the public, and will be found only in some minute points to differ from the account of Mr. Lawrence. Our author considers the operation of Gimbernat, which Richter and Hey stated to be most efficacious without knowing the anatomical reason on which its propriety is founded, to be the best and safest hitherto practised. We have already expressed our opinion that the effect of the operations, of Gimbernat and Cooper is precisely similar. We profess no decided preference on the ground of experience; but we certainly are disposed to admit the validity of some objections urged by Mr. Cooper to the division of the crescentic tendon, as well from their strong theoretical probability, as because they have never received confutation.

Mr. Lawrence entirely overlooks the existence of a fascial stricture, or one inferior to that produced by the crescentic tendon. The crural aperture is of a definite and permanent size, and generally sufficient to admit the point of the finger; but the fascial sheath, which has only such minute and delicate foramina as serve for the passage of lymphatic vessels, requires to be protruded, as we formerly observed, before the hernia becomes apparent. Hence a resistance is opposed to it in the second stage of the descent, which it did not experience in the first; and it will therefore be most liable, if inflammation supervene, to experience a stricture at this part. But when the opening thus formed in the side of the sheath has been dilated to a larger diameter than that of the tendinous ring, the bowel is invited to descend in larger quantity, and it goes on to dilate (by comparison) *indefinitely*, so that the crural foramen which has a tendinous and fixed boundary, becomes plugged, and is, in its turn, the seat of stricture. Accordingly, the fascial and the tendinous strictures, though in some cases they may co-exist, are most frequently alternated or produced under opposite circumstances, and as in the *large* merocële the more interior dilatation is required, so the division of the tunica propria and mouth of the sac have been repeatedly found efficient in *small* ruptures, which are confessedly most common in this species. The operation of Gimbernat recommended by our author in all cases, has in such to contend with the objection of superfluity, in addition to those of invariable difficulty and frequent danger.

Between the umbilical hernia of infant and adult subjects, a very essential difference exists. "This distinction" says Mr. Lawrence, "is derived from the tendency to contraction in the tendinous ring. In the former case, a radical cure is easily obtained; in the latter, it is nearly impossible. In the one instance it is sufficient to keep the viscera within the abdomen, and the ring will contract of itself. In the other the opening remains, whether it be occupied by protruded viscera or not. Hence also it follows, that practical observations drawn from one form of the complaint, cannot be applied to the other." Desault's very ingenious arguments in favour of the ligature, and the facts upon which they rest, are extracted from the Parisian Chirurgical Journal, and presented to the reader in an English dress.

A girding or constriction of the herniary sac frequently occurs in the congenital species *. Mr. Lawrence adds a case of this description to those already recorded.

A very curious case of artificial anus, alluded to in the body of the work, is minutely detailed in an appendix. The subject wás a healthy man about sixty years of age, and the disease of seventeen years standing. We offer no apology to our readers for the length of the following extract:

> "A prolapsus of the intestine has taken place through the artificial opening. The projecting part varies in length and size, at different times. It was four inches long when I saw it; and the basis, which is the largest part, measured nearly six inches in circumference. This prolapsus never recedes entirely, but is sometimes considerably smaller. It has occasionally protruded to the length of eight or ten inches, being at the same time equal in size to the fore arm, and bleeding copiously. This is attended with great pain, and only happens when the bowels are much disordered. Warm fomentations, and a recumbent position, relieve in this case, by causing the gut to return. The projecting part is of an uniform red colour, similar to florid and healthy granulations. The surface, although wrinkled and

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* Mr. Lawrence cannot understand the reason of Mr. Pott's substitution of the term *congenial*, for Haller's term *congenital*. We admit with him, that the appellation is absurd according to common acceptation; but a stronger authority in the use of words than reputable usage is etymological history. *Genial*, and *genital*, have the same derivation and are in one sense the same, viz. that pertaining to propagation and birth.

irregular, is smooth and lubricated by a mucous secretion. It feels firm and fleshy, and can be squeezed and handled without exciting pain. It approaches, on the whole, to a cylindrical form, and its anterior or loose extremity contains the opening through which the stools are voided. The person does not possess the slightest power of retaining his fœces, and his bowels are strongly affected by slight purgatives."

We cannot close this work without expressing our conviction of the superiority of the author's claim as a probationary essayist; and our opinion that this is one of ther are instances, in which the honour of a prize is reciprocally shared by the candidate and the college. This treatise merits in an eminent degree the proverbially powerful recommendation of the *multum in parvo*. It were to be wished, that upon all the great operations of surgery, the student had such a manual to resort to. Mr. Lawrence is entitled to the highest praise for diligence and accuracy, and his style in the detail of symptoms deserves to be noticed as peculiarly excellent. He has observed, perhaps judiciously, a gradual advance to public notice; and we shall be happy at a future day to congratulate him on the development of original matter, as valuable as that which he has translated with fidelity, and compiled with judgment.

Art. III. *Observations on the Preparation, Utility, and Administration of the Digitalis Purpurea, or Foxglove, in Dropsy of the Chest, Consumption, Hemorrhage, Scarlet Fever, Measles, &c. including a Sketch of the Medical History of this Plant, and an Account of the Opinions of those Authors who have written upon it, during the last thirty years.* By William Hamilton, M. D. Physician, Bury St. Edmunds, Suffolk. 8vo. pp. 214. Longman, and Co. 1807.

IN the year 1775, Dr. Withering, a physician of Birmingham, borrowing the hint from a family recipe, began to prescribe foxglove in dropsical diseases. The anti-hydropic efficacy discovered in the new medicine was transferred from one to another, until it reached the ears of Dr. Darwin, who finding that it deserved its character, published in 1780 a few successful cases in his son's posthumous essay. In the mean time Dr. Withering had been employed in carefully administering digitalis, and attentively observing its effects, and after subjecting it to a ten years' trial, published a work strongly recommending the use of this medicine in ascites, anasarca, and hydrops pectoris. About this time Dr. Darwin inserted a paper in the Transactions of the

College of Physicians, in which he relates several cases of dropsy, and one of consumption, which had been cured by digitalis. If experience had proved the efficacy of this medicine in pulmonary consumption, this would have furnished one instance of mere speculation leading to practical advantages, as Dr. Darwin had been led to prescribe foxglove in phthisis pulmonalis, by the supposition that the ulcer of the lungs could not heal whilst pus continued to flow from it; that this would cease as soon as the absorption equalled the secretion of the ulcerated surface, and that this equalization might probably be effected by a remedy which had so remarkable a power over the absorbent vessels. It was soon discovered however, that although foxglove possessed great power over the lymphatic, it exercised a much more extraordinary one over the arterial system; diminishing the rapidity of the circulation in a very considerable degree. In 1792, Dr. Ferriar of Manchester, related four cases of pulmonary hemorrhage, which were much relieved by the infusion of this plant. In 1799, Dr. Beddoes assisted to extend the antiphthisical reputation of digitalis, by the publication of his Western Contributions, although his praises of the medicine in cases of consumption were founded not upon his own experience, but upon that of his correspondents. Dr. Beddoes himself had met with little encouragement, but Dr. Fowler and Dr. Drake had apparently succeeded in curing several consumptive patients by foxglove.

Every physician must have a theory, and Dr. Drake had his. The hectic of consumption (according to him) arose from the absorption of oxygenated pus. A medicine therefore, which would produce the absorption of the pus before it had time to become oxygenated, would remove the cause of hectic fever, and such a medicine was digitalis. A short time after the Western Contributions, appeared the Essay on the Causes, early Signs, and Prevention of Consumption. In the interval betwixt these publications, Dr. Beddoes had met with great and unexpected success in the administration of foxglove in phthisis, and therefore again appeared before the public, stating his success, and painting our future triumphs over the disease in the vivid but fading colours of his eloquence. The attention of the public seems to have been completely roused by these addresses, and the Medical Journal teemed with communications exhibiting various degrees of success. Dr. Ferriar about the same period published his pamphlet on the foxglove, in which he depressed our hopes of its efficacy in consumption, but raised them in regard to its use in inflammation and inflammatory fevers. Then arose the question, is digitalis a direct sedative? and Dr. Beddoes and Dr. Kinglake refused to exhibit it in inflammatory diseases because they believed it to be a stimulus. In 1800, Dr. Mossman published his essay on scrophula and consumption, in which he speaks highly of foxglove in pulmonary consumption; and, in the same year, Dr. Drake through the medium of the Medical and Physical Journal, transmitted an important record of his experience. Of fifteen cases of phthisis pulmonalis, five in the purulent state, and four in the incipient recovered under the use of digitalis: one in the purulent state remained uncertain, and five in the same state died. Dr. Magennis, physician to the Naval Hospitals at Plymouth, used foxglove in his hospital practice with a success which we believe has not been equalled; and has detailed his experience in the Journal before mentioned. In 1801, appeared Dr. Beddoes' Essay on the Medical and Domestic Management of the Consumptive. In the use of digitalis his success appears to have fallen below the anticipated level; and if his former picture was enlivening, this was gloomy. If we pass over Dr. Reid's late essay on consumption, the next personage in the history of the medical use of digitalis, is Dr. Hamilton of Bury St. Edmund's, the author of the volume which lies before us for examination.

The first chapter contains a sketch of the medical history of digitalis, in which the opinions of medical writers from the time of Dr. Withering to the present, are detailed. This sketch is well executed, and will be found very useful to those who have no time or no inclination to consult the original writers from whom it was derived. We come next to a chapter on the cultivation and preparation of digitalis; the sum and substance of which is, that the plant which grows in gardens is as good as that which grows wild; that to avoid inequality of power, it ought to be gathered when the flower stems have shot up; that when prepared, it ought not to be kept longer than a year; that the infusion is best for dropsical diseases, the tincture for inflammatory diseases, and the powder for less important diseases. A chapter follows on the exhibition of digitalis, with practical observations and reflections. Our author recommends Dr. Withering's infusion, which is prepared with one drachm of the dried leaves and eight ounces of boiling water; to which when strained, one ounce of any aromatic spirit is to be added as a preservative. Of this preparation from three to six drachms are to be given every eight hours. Nausea is not necessary to the diuretic effects of the medicine. If Digitalis fails when employed alone, it ought to be combined with mercury. Foxglove is more useful in hydrothorax than in any other dropsical affection. It is much less useful in ascites and anasarca; and in these diseases it much oftener requires to be combined with auxiliary remedies.

"I apprehend that the observation of Dr. WITHERING respecting the difficulty of *mildly* affecting dropsical patients of considerable irritability and tenseness of fibre, by the DIGITALIS, will also be found, in a great measure, to apply to the more gradual exhibition of the remedy, when it is only intended to reduce the velocity of the circulation. In a great majority of instances this effect of FOXGLOVE cannot be depended upon, and after the most cautious introduction of the Tincture, with every attention to the circumstances by which it's favourable action is promoted, the pulse cannot be diminished in frequency, before such hostile symptoms have appeared as compel us to abandon it's use; or, the circulation becomes slower only when these unfavourable consequences have taken place. An attentive consideration of the causes which seemed to prevent this operation of FOXGLOVE, by which I have so often been foiled, has convinced me that they are to be found in the presence of Inflammation, or of a tendency to inflammatory action, of the vascular system. In Dropsy, where this disposition is scarcely ever present, the circulation is commonly retarded by the use of DIGITALIS; and in Hæmorrhage, where this remedy produces more decided benefit, perhaps, than in any other species of disease, after the vessels have been emptied by repeated losses of blood, the depression of the pulse is almost invariable:—I presume, also, that in health no great difficulty would be found in causing this effect of FOXGLOVE. But in Pneumonic Inflammation, where the abatement of the impetus of circulation and the increased activity of the absorbents would speedily cure the disease, until considerable loss of blood have taken place, these favourable consequences of the exhibition of this plant will be looked for in vain. I am aware that authority of no mean weight can be adduced against this decision. I have, however, been too often disappointed in my hopes of sparing the further loss of blood, in inflammatory affections, and especially in Pneumonia, by the agency of DIGITALIS, not to be satisfied, that however respectable the Authors by whom it is supported, the opinion of the powers of this remedy over Inflammation is incorrect. Towards the latter period of Pleurisy indeed, when bleeding proportioned to the severity of the attack, and the strength of the patient shall have been premised, and when Vomica, notwithstanding, seems likely to take place; or, if great secretion of mucus be present, and effusion apprehended; this remedy will then be found to deserve the full extent of Dr. CURRIE's encomium upon it, prefixed to this volume."

The next chapter is on the use of Digitalis in hydrothorax. Here the author judiciously explains the difficulty of distinguishing this disease from other affections of the chest. He believes that the anasarca pulmonum is much more common than it is generally understood to be; and he advises the use of Foxglove in hydrothorax, anasarca pulmonum, and in cases of supposed organic affections of the heart, because he is of opinion that the symptoms commonly attributed to these affections, are often combined with, or even arise from dropsical effusion. The prudent physician will be cautious in making his prognosis with regard to any of these affections. Every man of extensive observation must occasionally have seen the fallacy of the common diagnostic signs of hydrothorax, not excepting the starting out of sleep with a sensation of suffocation. It is commonly believed that polypi of the heart are universally formed after death. We have however seen one case, and heard of others, in which the chief symptoms were dyspnœa, palpitation, and intermitting pulse, greatly aggravated by upward motion, and in which on examination after death, nothing could be discovered but a large firm polypus adhering to the inner surface of the left ventricle. It was not one of those long and slender coagula which are undoubtedly formed after death, but was large and firm, and like that described by Malpighi. No one can read the case related by Tulpius, without believing the possibility of these substances forming during life, and being the cause of dreadful and fatal symptoms. The reasons for supposing that in these cases the symptoms arose from polypus of the heart, are shortly these; that no other cause of the symptoms could be discovered, and that this cause was sufficient to explain them. How these polypi are formed, whether by coagulation of the lymph of the blood, or by an effusion of coagulable lymph from the internal surface of the heart, is another question, which we shall take an opportunity of discussing in another place.

The chapter on Phthisis Pulmonalis contains nothing new. Dr. Hamilton thinks that Foxglove is useful only in the infancy of the disease. He recommends it in hæmorrhages after blood has been lost, the pulse has become soft, and all inflammatory diathesis has ceased, when he thinks it will seldom fail. He advises the exhibition of the medicine also in the anasarca which follows scarlet fever, and in the latter stage of measles.—" It is in the advanced stages of measles, (he writes) when the disposition of the membranes lining the chest, to inflammation shall have ceased, or have been overcome by appropriate remedies, that it will be found most effectual in checking the consumptive tendency."

The volume concludes with an appendix containing six cases of hydrothorax, successfully treated by digitalis; a reply to some objections stated by neighbouring practitioners to the use of the medicine in this disease, and a communication from

Dr. Maclean, practitioner at Sudbury in Suffolk. This communication consists of cases of hydrothorax which were cured by digitalis and calomel, and some general observations on the use of the former medicine. Dr. Maclean has met with some cases of hydrothorax in peculiar constitutions which were not as usual relieved by this medicine.

" If, in addition to these, the limbs be of a dark livid hue; the patient of a full corpulent, or what is termed a *cold phlegmatic habit;* the bowels costive, more especially if they require the aid of active purgatives; very little, if any, advantage is to be derived from the foxglove, either alone or in combination, in the fullest doses, and administered with the usual precautions: on the contrary, by the distressing sickness, and other unpleasant symptoms which it produces, it seems to retard the salutary operation of other remedies. It is proper, however, to remark, that when the disease occurs in such habits, the cure is in general tedious and difficult. Experience therefore has taught me not to build my hopes on the foxglove alone; and if I have had more than ordinary success in the cure of this formidable disease, it is to be attributed more to the united powers of the combinations used, than to any individual ingredient entering their composition. Digitalis, squills, calomel, crystals of tartar, the fixed vegetable alkali, and a few other diuretics, when separately administered, will very seldom be found permanently to cure the disease; but by selecting, blending, and combining them together in certain proportions, according to the particular circumstances of every case, which in no two instances are precisely alike, each of these seems to acquire additional efficacy, and the most salutary advantages will result; and certain peculiarities of constitution will be found, in which some of these cannot be administered. Hence the necessity of the knowledge of the art of judicious selection and combination, which can only be acquired by long experience, aided by a close and patient attention to the operation of medicine, and the phenomena of the disease. I shall only observe, that by means of the combinations I have been in the habit of using, my object invariably has been to accomplish at once, or by one indication, what is generally attempted by three. Thus, while administering remedies for evacuating the water, the removal of any organic affection, or other cause which may have occasioned its effusion, retard or prevent its absorption, as well as the restoring the lost tone and strength of the habit at large, or of any particular organ, which generally forms a prominent feature of the disease, are held in view at the same time: Hence, as these vary in degree and kind in every case, so those combinations must be varied also in the same proportion; and instead of decreasing the degree of debility already existing, the strength very generally improves from the beginning."

Our readers will now be able to judge whether or no we are accurate in saying, that the important materials from which this book is made, ought to have been compressed into an essay for the Edinburgh Medical Journal. We agree with Dr. Hamilton that foxglove is a powerful and valuable medicine, and are quite willing to allow that this volume contains important practical observations very well stated and very aptly illustrated, but at the same time we contend, that they might have been stated in a smaller compass, and would thus have gained a far more extensive circulation.

Dr. Hamilton can claim no credit either for conciseness or novelty. His practical observations may be important, but they are old; yet we are not for this reason inclined to undervalue the utility of his labours. The medical profession is far from being filled with reading men. When therefore a new idea is emitted, it escapes many, strikes feebly upon others, and produces its proper effect upon very few. An important practical observation ought to be stated again and again. The repetition increases the quantity of evidence for the accuracy of the fact, impresses its importance on minds which it never would otherwise have reached, and renews impressions which time may have obliterated.

Art. IV. *An Essay on the Diseases incident to Indian Seamen, or Lascars, on long voyages.* By William Hunter, A. M. &c. &c. Folio, pp. 236. Calcutta, 1804.

On the subject of tropical diseases we have already several valuable works in the English language, and many important practical improvements have been introduced into this country, by men who had passed a very early part of their lives in climates very different from our own. It would be a valuable addition to our stock of knowledge, could we procure a faithful history of the diseases to which men are more peculiarly exposed in every variety of climate; and a set of medical travellers who should describe the distempers most prevalent in different latitudes, and in different states of society, would contribute in no slight degree to the advancement of our science. It cannot be doubted that both the physical and moral nature of man is modified by situation, and that the diseases of the body like the passions of the mind, acquire a greater degree of violence under the influence of a burning sun, and require more prompt and powerful remedies. As we approach the polar regions, man becomes a phlegmatic and sometimes nearly a torpid animal; and the diseases to which he is liable are altogether dissimilar from those which prevail within the tropics. The plague and the yellow fever seem confined within a certain range of temperature; the latter certainly has never been known to prevail when the thermometer was below 80° of Fahr. and the extreme heat of summer in Egypt always puts a stop to the ravages of the plague. Scrofula seems confined to the natives of colder climates, and though it may be continued for one or two generations in warm latitudes, it gradually dies away. Education, religious and political institutions, and different modes of life, modify the effects of climate; and the army physician who is to guard against disease in different parts of the world, should attentively study the customs and the remedies employed by the nations among whom he may chance to be thrown. In the East Indies it is well known, that the native physicians are acquainted with several powerful vegetable remedies. Dr. Buchanan has lately published an account of an Indian medicine, which after repeated trials has proved uniformly successful in the cure of the tœnia; and Dr. Baildon mentions a valuable remedy called the oc-kow, which the Chinese employ in some complaints of the breast. It is satisfactory to find, that the surgeons in the service of the East India company are beginning to attend to the study of natural history, and to investigate the nature of several peculiar diseases which occur in our oriental territories. In England the number of medical publications which annually issue from the press might be diminished with advantage to the public; in India on the contrary we recommend every practitioner to publish the result of his experience, and as far as we are concerned, we promise that he shall be very civilly treated. Mr. Hunter, the author of the present volume, was employed in his capacity of marine surgeon to the company, to investigate the cause of the very great mortality which prevailed among the Lascars in some of the country ships returning from England. Besides the ordinary diseases which occur on ship-board, a peculiar distemper, which Mr. Hunter calls anasarca cacotrophica, proved by far the most destructive. As the most valuable and interesting part of the present volume relates to this disease, and to one of a nature very similar (if not precisely the same) which is endemic in Ceylon, we shall confine our remarks chiefly to this subject. The surgeons of the different ships in which this species of anasarca prevailed, vary somewhat in their descriptions; probably it did not always assume exactly the same character, though there is enough of resemblance to justify similar medical treatment. Mr. Hunter has drawn up his account of the anasarca cacotrophica from the different descriptions of the naval surgeons, and has given their testimonies in the appendix. The summary we shall extract.

"The disease commenced with œdematous swelling of the feet, accompanied with stiffness and numbness of the joints. These two symptoms appear to have been so nearly simultaneous, that it is difficult to say which of them preceded the other. In some instances, an aversion from motion was observable for a day or two, before the appearance of those complaints; in others the men continued their usual exercise and duty, while the swelling was confined to the feet. But it rapidly extended upwards, occupying the legs and thighs. When it reached the abdomen, it caused severe dyspnœa, which increased to such a degree that the patient seldom survived one day, after the distention had extended as high as the stomach. In one instance, the fatal dyspnœa was not attended with any perceptible swelling. Death was preceded by pain at the scrobiculus cordis, increased by pressure; and about this time many had bilious vomitings. The face was swelled and bloated. Thirst great during the whole disease, but much increased a little before death. Urine scanty, and voided with difficulty. They were in general costive. No account is given of the state of the appetite, nor of the pulse.

"None of the cases exhibited any swelling, sponginess, or bleeding of the gums; or any spots or sores that could be deemed scorbutic, on the limbs, or any part of the body. Some indeed of the crew of the *Arran*, had sores proceeding from an itchy, or herpetic affection; but those appear to have rather served as a salutary drain, than to have formed a part of the disease. The whole duration of the complaint from the first seizure to its fatal termination, was often comprised within the the space of two days: and it appears to have seldom exceeded twenty. In the only dissection which was made of the dead, a small quantity of water was found in the cavity of the abdomen; and the cellular substance all over the body was greatly distended with the same fluid. The cavities of the chest and of the pericardium did not contain more than the usual quantity. The viscera of the thorax and abdomen were perfectly sound."

Mr. Ross in his account of the diseases which prevailed on board the ship Porcher, mentions the presence of scurvy, and describes the symptoms of that disease as it occurred on board his ship.

"Sixteen of the lascars were attacked with the scurvy, five of whom died before we made the Cape: the symptoms appeared as follows: feet and legs became swelled and pitted on pressure, with petechiæ all over them; general lassitude, accompanied with great disinclination to motion; gums looked livid and were tender to the touch at first; the progress of the disease being rapid, the abdomen soon was affected with difficult respiration; the aversion to motion increased as the swelling of the body advanced; and towards the latter stage of the disease the gums became spongy and foul, urine scanty and high coloured, face, arms, and hands became considerably swelled, and gave a degree of resistance to the touch; death shortly closed the scene.

"On opening two subjects, who were attacked in the manner described, and died of the disease, there was a considerable collection of fluid in the cellular membrane, but no unnatural appearance in the thoracic or abdominal viscera. With respect to the mode of treatment, the remedy administered first was a purgative, composed of calomel and jalap, and after its operation, an opiate; one pint of beer in a sound state. a few plumbs, sugar and tea was daily given to those who were ill, which had the salutary effect of arresting the progress of the disease until fresh vegetables were procured: some of them refusing to take the beer, &c. fell victims to their obstinacy. On the first of August we reached *False Bay*, where we were abundantly supplied with oranges, which were the only vegetables procurable at the season, in any quantity. At this time, ten of our people were confined to their hammocks unable to move without assistance; breathing laborious, great languor, body and extremities considerably swollen: by the use of the fresh oranges in three days after our arrival, those men were able to sit up, a short time in the day. On the 7th, which was the day we sailed, they were so far recovered as to walk about the deck a little, and in two weeks after our sailing they were perfectly recovered, and able to return to their duty. No symptom of the disease re-appeared in the voyage."

The great resemblance between the anasarca cacotrophica and scurvy as it is here described, must be evident at the first sight. In the narrative of Mr. M'Wirther, surgeon to the Scaleby Castle, it is stated that they suffered much from scurvy which varied in the violence of the symptoms, but that dyspnœa was observed in all the cases, and generally, swelling of the lower extremities; the duration of the disease was by no means uniform; some were carried off very suddenly; others survived four, five, and nearly six weeks.

Mr. Christie's description of a disease called beriberi very prevalent among the troops quartered at Trincomalie, bears a striking resemblance to the anasarca cacotrophica, and to the scurvy as it appeared among the lascars.

"The beriberi is a disease of a peculiar nature, which has been extremely frequent and fatal amongst all the troops, both Europeans and natives in this settlement.

"In the milder cases of the disease, the patients are first attacked with some stiffness of the legs and thighs, and this is succeeded by numbness and œdema, sometimes paralysis of the lower extremities

In the course of a few days, if not prevented by medicine, these symptoms are succeeded by swelling of the whole body, attended with a sense of fulness of the belly, and more particularly with weight and oppression at the præcordia, dyspnœa, starting in the sleep, and all the usual symptoms of hydrothorax. In the latter stage, the dyspnœa and anxiety become extreme, the uneasiness at the epigastrium increases, attended with almost constant vomiting, and occasionally spasms of different muscles; the pulse becomes very feeble, the lips and countenance livid, and the extremities cold. Some fever with delirium often now accedes and terminates the life of the unfortunate sufferer. In the more sudden and severe instances, the patients from the first complain of universal debility, and extreme oppression, anxiety and dyspnœa. In some of these instances, the progress of the disease is so rapid that it carries off the patient in six, twelve, twenty-four, or thirty-six hours after its first attack; more frequently however, its duration is for several weeks. In a few cases where the disease was no less fatal, there was not any swelling observable externally; but the patient with the other symptoms had evidently the bloated leucophlegmatic face of a dropsical person.

"Upon dissection of different subjects who had died of this disease, more or less water was found in one or all the cavities of the chest; most commonly in the pericardium, but in general, more inconsiderable than might have been expected from the violence of the symptoms. The cellular substance surrounding the heart was in some instances loaded with water; and the heart seemed in two or three cases of an uncommon size. In one instance, in which the progress of the disease had been very rapid, I found a large coagulum of lymph in the right auricle. The cellular substance of the lungs was in many cases loaded with water. In a few cases also, there was water effused in the cellular substance on the surface of the brain; and in one instance, more than an ounce of water was collected in the ventricles. In most cases, water was found in the abdomen, and cellular membrane throughout the body; and in many subjects there was a remarkable obesity, even after a long continuance of the disease, and of the use of mercury, antimony, and other powerful medicines. Men of every constitution are occasionally attacked with the beriberi, but the aged and debauched seem to be most liable to it; and men who have once had the complaint, are the most subject to it in future. I have remarked that a very great proportion of the patients seized with this disease, were men who were accustomed to lead a sedentary and debauched life, such as taylors, shoemakers, &c. who when working at their trade are often excused the duty of the field, and by their double earnings, are enabled to procure a larger quantity of spirits than the other men. I have never met with an instance of this complaint in a woman, an officer, or a boy under twenty; although persons of every description, seem equally liable to the other diseases of the place, such as fever, flux, or liver complaint.

"It would appear, that a stay for some months on the station, is almost essential for the production of the disease; and that the greatest predisposition to it exists, when troops have been about eight or twelve months in the settlement.

"The 72d Regiment, and Coast artillery, landed here in July, 1795. The beriberi was with them most prevalent in the autumn of 1796; but they had little of it in March, 1797, when it was extremely frequent with the 1st battalion European infantry, who had arrived here in August 1796.

"The 80th Regiment relieved the 72d in March, 1797, but suffered little from the disease, till the November following. The Honourable Company's Malay corps arrived here, from Jaffnapatnam, in June, 1797; but the complaint did not appear amongst them till the January following, when it became very frequent and fatal. Two hundred drafts joined the 80th at Trincomalie, on the 3d of January, 1798; but none of these men had the disease in January, February, or March, although it was then very frequent with the other men of the regiment; since that time, however, these drafts have been at least as subject to it as the other men."

That there is a great affinity between this disease and scurvy, cannot be denied; yet, according to Mr. Christie, acids, which are among the most powerful remedies in scurvy, were not successful in the cure of beriberi. Mr. Colhoun, with no other medicines than lime-juice, arrack, and nourishing diet, cured ten patients, who were affected with this complaint on board the Stirling Castle, the crew of which were afflicted with the anasarca cacotrophica; vitriolic acid and spruce beer proved the most effectual remedies. Whatever induces debility, be it cold, poor living, idleness, despondency or fatigue, seems to have rendered the system more obnoxious to the disease called beriberi, and the same causes, on board ship, induced the anasarca cacotrophica. The analogy of both to scurvy is so considerable, that we think it ought to influence the medical treatment. Mr. Hunter conceives, that the absence of spongy gums and of petechiæ, suffices to prove the difference of the diseases. On board Lord Anson's ship, which probably suffered more from scurvy than any other vessel upon record, spongy gums were often absent even in the last stages of the disorder, and Lind records many similar instances. In the account of the diseases which appeared on board the Porcher, all the symptoms, which constitute the anasarca cacotrophica, were present, with the addition of petechiæ and spongy gums. The petechiæ cannot be considered as the pathognomonic symptom, the absence of which would imply the absence of the disease. It is, indeed, a matter of trifling importance, where we place this disease in the nosology, provided we can lay down effectual rules for its treatment.

Mr. Hunter argues in the present publication, as if the remedies to be employed varied greatly in the two diseases, and consequently that it became highly important to determine whether the beriberi should be placed among dropsies, or be considered a species of scurvy. To us it appears that in either case, a class of remedies might be employed applicable to both diseases. We would particularly recommend the crystals of tartar, a remedy which does not seem to have occurred to our Indian brethren, and which, both by its diuretic and acid properties, seems calculated to answer a double purpose. In other respects, the treatment seems to have been judicious: fresh provisions, change of situation, and nourishing diet, seem chiefly to have been relied on. We have read the present volume with considerable interest, and can strongly recommend it to those young men, who are about to engage in the service of the East-India Company.

Art. V. *A Dissertation on Gout; exhibiting a new View of the Origin, Nature, Cause, Cure and Prevention of that afflicting Disease; illustrated and confirmed by a variety of original and communicated Cases.—Additional Cases of Gout in further proof of the salutary Efficacy of the cooling Treatment of that afflicting Disease, with Illustrative Annotations in its Support, Controversial Discussions, and a View of the present State and future Prospects of the Practice.* By Robert Kinglake, M. D. &c. 8vo. pp. 348 and 412. Taunton, Poole; London, Murray, 1804 and 1807.

SINCE the distinguished era when observation and experiment became extensively introduced into scientific reseach, medicine has received accessions of information the most valuable and important. It has not however, advanced in the career of improvement with a rapidity proportionate to that of other branches of science; and it has not like them, escaped controversy on occasions where experimental scrutiny in its utmost severity might be exercised: indeed, controversy was not more violent, arrogant and tenacious, "when moist and dry held everlasting war," and when syllogism with its multiplied imperfections, was alone employed to terminate the contest. This, no tenderness for the honour of inductive philosophy need tempt us to conceal, for the admission is no impeachment of its excellence: the ambiguous, intricate and mysterious nature of those events which are the basis of legitimate medical inquiry, and which constitute its evidence, is at once a sufficient explanation and defence.

The necessity of a continual appeal to facts in every attempt to establish general principles, we do most thoroughly acknowledge, but we do not conceive that correct opinions in medical science can ever prevail, whilst narratives obviously partial, obscure and distorted, shall be accepted as adequate data. Facts of this description are adverse to the progress of science, as the judicious and reflecting part of the profession must often have occasion to feel; it is through the unguarded admission of whatever is confidently alleged to have happened in the manner and from the cause which the asserter inclines to maintain, that the most absurd doctrines obtain extensive adoption, the most glaring impositions a high degree of public renown.

It is greatly to be wished that we were in possession of precise rules for determing the validity of medical evidence: this however is more to be desired than expected. Without attempting to discover such rules, which would open an inquiry far too extensive for our present undertaking, we may with advantage remind our readers in the language of a very intelligent writer, that "the evidence requisite to prove or disprove any proposition in the science of medicine is of a peculiar kind, differing materially from that species of proof which satisfies a court of law. Both direct and circumstantial evidence which would leave no doubt in the breast of judges and juries, have often not the slightest tendency to render a medical fact even probable. The declarations and even oaths of the most conscientious, disinterested, and able men, are all insufficient."

Of the difference and the difficulty here stated in the application of facts, a satisfactory account may easily be offered. Every fact relating to the cause or to the cure of diseases comprehends, in a greater or lesser degree, opinion, and this opinion few men, whatever may be their powers of discrimination, can separate from the part which is strictly fact. When, for example, it is asserted that a disease has been removed by the application of any particular medicine, a fact is supposed to be advanced which every person of correct sensations is competent to establish; yet two opinions are involved in the allegation, which it is sometimes impossible and always difficult completely to support: the first opinion is that the disease is that, and that only, which is mentioned; the second, that it has been removed by the medicine, and by the medicine alone. When masses of inert, unorganized matter act on each other, the circumstances and the result are so simple and so obvious, that no ambiguity respecting the cause and the correspondent event can arise: hence the indisputable demonstrations, and the incontestible principles of mechanical philosophy. But where vitality has been superadded to organization, and that magnificent appendage to the human fabric by which it is at once distinguished, informed and exalted, has likewise been conferred, the utmost perplexity obtains. The combined power of these principles, the vital and intellectual, suspending, opposing or co-operating with the means which art employs, often produces the consequences wholly or in part, which we by ignorance inevitable, ascribe to the means alone: it becomes at one time a preserving, at another a healing power; protecting the system against that destruction which error might have produced, or removing that malady which the incompetency of art might have suffered to prevail. Hence we may understand why so many articles in so many disorders, though possessing opposite qualities, are found to be safe, efficacious or even infallible; why acid and alkali, stimulant and sedative, heating and cooling means, in the very same disorder have obtained the most strenuous advocates and the loudest applause; and hence we may perceive why observation and experience should have authorized Dr. Heberden to assert, that "new medicines and new methods of cure perform *miracles* for a while."

Into the foregoing train of reflections, we have been led by an attentive consideration of the various opinions and practices which have been proposed for the explanation and cure of gout, with particular reference to a review of the writings of Dr. Kinglake, and those of his opponents and defendents. This controversy, in which each party claims exclusively, the merit of using for their weapons fact and experience, is unquestionably of great importance; but the manner in which it has been conducted we must with little exception reprobate, as it is far more personal than philosophical: and be our decision what it may in regard to the state of the gouty system, we must declare that the *humoral* pathology has flourished in its worst form, and that *morbid acrimony* has prevailed with its most mischievous and virulent activity.

It is worthy of remark that two of the latest and *best established* attempts to cure the gout, (with the exception perhaps of the Rev. William Barclay's antibilious pills,) have originated from early and peculiar determinations of mind: thus from Dr. Kinglake we derive the following valuable information.

"My earliest independent speculations on the nature of disease, (now nearly twenty years since) were founded on temperature and its occasional variations. The *obvious expansion and contraction* which accompany increased and diminished heat were in my judgment of the most *extensive and important application in the doctrine and cure of diseases.* But the theory of repulsive motion, as explained by the ingenious Mr. Humphrey Davy (in his Essay on Light, Heat, &c.) in dejection of material heat, has contributed greatly to elucidate and confirm my previous view of the subject." Note, page 78.

Mr. Welles the other great discoverer before hinted at, "in the retrospect of his life, finds that he had an early aptitude to attempt to discriminate some of the more obvious qualities of vegetable substances by the appearance they exhibited to the eye:" this gentleman we trust will illuminate the world by his speculations on "the cause of this propensity," and "shew the degree of perfection which it attained," by pointing out what must be of *incalculable advantage,* "the mode of determining whether a tree bore sweet or sour apples, &c. by the configuration of the leaf or the twig:" and Dr. Kinglake will surely not withhold the precious fruits of his lucubrations, the promised *Thermonosologia,* which have been maturing ever since the first glimmerings of that intellect, now blinding us by its meridian splendour.

It will or *ought* to be remembered by our readers, that the first annunciation of Dr. Kinglake's "New View of the Nature and Cure of Gouty Affections," was made through the medium of the Medical and Physical Journal, *in the year eighteen hundred and one,* conferring like the planet Piazzi, an extraordinary dignity and lustre on the *infant century.* This communication, with a second transmitted through the same channel in the following year, is republished with several additional articles extracted from the same periodical work, as an appendix to the first systematic treatise in elucidation of the *new views,* entitled "a Dissertation on Gout." Respecting the objects of the author, we obtain immediate information, for in the first page of the preface the following passage occurs:

"Whatever obloquy may befal my endeavours to correct the errors of science, and of vulgar adoption on this subject, it will not repress my ardour for establishing the truth of *positions* so important as that gout differs in no essential circumstance from common inflammation; that it is not a constitutional but merely a local affection; that its genuine seat is exclusively in the ligamentous and tendinous structure, that its attack is never salutary; that it should neither be encouraged or protracted; and that if seasonably and appropriately treated, it is as easily remediable as inflammatory excitement on the muscular, cuticular, or any other description of organic texture."

Were we not restrained by a certain indescribable dread of being convicted of an attachment to the humoral pathology, that greatest of all possible crimes, we should hazard the expression of an apprehension, that Dr. Kinglake and several of his correspondents are not really acquainted with the disease on which they have written. In a perusal of the second section, purporting to treat "of the nature and *constitution* of gout," we have sought in vain for circumstances essential in the general train of symptoms denoting the disease; and have found affections such as rheumatism, white swelling, gout, sprains and common inflammations, declared to be *fundamentally the same;* affections, which a degree of ignorance that it would not be safe for us verbally to characterize, or blindness "structural" could alone confound. It may perhaps be alleged that this chapter does not profess to convey a history of the disease; but somewhere certainly "*the strict podagral character of arthritic affection*" ought to have been dispalyed, and no division appears to us more suitable. When we refer to the descriptions of Hoffman, Cullen, and Darwin, whose intellects the abhorred "*materies morbi*" cannot be supposed to have vitiated, we shall find that although the inflammatory affection is *sometimes* what Dr. K. assumes it *always* to be, sudden and primary, yet, that in the greatest number of cases it is preceded for several days, by a degree of torpor of the whole body, and in particular, by disturbance of the functions of the stomach; by an unusual degree of coldness of the feet and legs, and a suppression of their cutaneous discharge; by numbness alternating with a sense of prickling in the lower extremities, frequent cramps and unusual turgescence of the veins. It is further of especial consequence to observe that with the formation of the paroxysm, these constitutional symptoms are known to decline, and that when the disease ceases entirely after remaining for some time in a joint, it generally leaves the person in perfect health, enjoying greater ease and alacrity both of body and mind than had for a long time been experienced. This, Dr. Heberden who is not disposed to admit the curative influence of the gout, allows to have been the opinion of about one third of the number of those afflicted with the disease whom he had attended. Observation, as extended and as unexceptionable as Dr. K.'s, although not detailed with the same parade of cases, has likewise shewn, that where the inflammatory symptoms have suddenly ceased, severe constitutional derangement has ensued; and for these reasons, with "the senseless inflexibility of habitual prejudice," and "the stupid habit of unintelligent adoption," do we still persist in our opinion that the disease taken as a whole, is not confined to the parts where by external appearances its presence is most strikingly manifested.

As we wish that our readers should be informed why the causes productive of gout, which are well fitted to excite constitutional derangement, should notwithstanding affect exclusively the most distant and least susceptible parts, we insert the following passage:

"Various causes connected with temperature, diet, and constitution, operating on systematic excitability, may from accidental preponderance of motive susceptibility on the ligaments and tendons, finally exert a concentrated or inflammatory influence on these parts, and thus induce the formal character of local gout. It must not then be contended, that the gout constitutionally existed before its appearance in the affected parts, as that would involve the solecism of giving being to a nonentity."

It will not we trust be supposed that to quote, implies to understand what is quoted: from the suspicion of such presumption *we* wish to be preserved; we, who in mute astonishment, and comparative littleness, filled with veneration the most profound, have gazed upon the sublimely mysterious pages of Dr. Kinglake. We know, that "when we cope him in his darkest moods, he must be full of matter," and therefore we present passages which to us are sealed, to such bright intellects as are qualified to unfold them: in verification whereof, we introduce the following paragraph, satisfied that wheresoever found, its illuminating influence will be hailed with rapture. "*Animalized matter derives from organic arrangement, active powers, evolvable form, and only tenable with that peculiar fabric.*"

Having shewn in what manner the causes operate which induce the formal character of local gout, we shall complete the account by the following descriptive statement of the circumstances which enable them to produce a disease, "whose very dalliance is torture, and whose frequent embraces are more odious than death."

"The *dense compactness* of this fabric (the ligamentous and tendinous,) gives to its nervous, vascular, and cellular substances, sentient, irritative, and resisting powers, peculiarly adapted to induce the painful conflict sustained, when from any cause these parts are subjected to inflammatory violence. Strong derivant excitement, impulsive afflux of fluids, and unyielding contractility of vessel, are sufficient to furnish the most distressful phenomena of gout."

In the spirit of dullness and humility unfeigned, we implore Dr. Kinglake, if the love of the unintelligible should so far prevail as to require a republication of his work, that he will condescend

descend to instruct us in what manner "*dense compactness*," that is, *dense density*, or *compact compactness*, can operate in giving *sentient* and *irritative* power to the parts where it prevails?

That we may not be accused of imposing on Dr. Kinglake the alleged identity of sprain, rheumatism, gout, white swelling, and all the forms of phlegmonous inflammation, which our surgical readers may think *rather* absurd, we deem it requisite to allow him to appear in our vindication:

"Ligamentous excitability, stimulated to the extent of inflammatory action, may assume different forms, but changes not its fundamental nature. In the lumbago, or disease of the lumbar vertebral ligaments, the sciatica, or hip gout, the white swelling, or ligamentous inflammation and thickening of the knee and other joints, are various examples of inflammatory excitement of the same structure as that which is the seat of gouty irritation; and these acknowledge no other difference than what consists in degree and situation." p. 16.

But even this *magnitudinal and positional* defect of identity must be rejected, and the identity be admitted without qualification, provided we may finish the syllogism, of which Dr. K. has furnished the first and middle terms. "It may then be affirmed, as has been already incidentally hinted, that

"No difference whatsoever exists between gout and other forms of inflammation, but in circumstances of degree and situation;" p. 30. but,

"Inflammatory excitement is universally similar, whatever be its degree or situation;" p. 38. ergo,

Gouty and other forms of inflammation are absolutely and unconditionally the same.

Let a surgeon, who has casually learned or personally felt, that gout often leaves the part which it first attacked and fixes on another, refer to his experience, to determine whether he has ever seen the wrist in sympathetic accordance with the ancle, suddenly sprain whilst the patient sat at rest in an easy chair: should he decide in the negative, Dr. K. will demonstrate to him, that the truth had been obscured, by unwholesome exhalations from the humoral doctrines, and by purging his visual ray will exhibit to him the truth of the following assertions:

"The inflammation induced on the ligaments and tendons by external violence, and which is significantly termed sprain, discovers as strong an identity with gouty irritation as occurs in rheumatic affection. The same parts are subject to inflammatory excitement, and the same effects are consequently manifested. Exquisite pain, immobility of the affected joint, efflorescence, or shining tumefaction, *associative affection of other joints*, and more or less of systematic irritation, at once characterise ligamentous and tendinous extension, called sprain, and present an imposing catalogue of either gouty or rheumatic symptoms." P. 15.

Of the occurrence of white swelling in a part, in consequence of being "*associatively bound in indissoluble participation*," no evidence occurs, we make bold to maintain, in any writer from Hudibras to Hunter!!

In the first part of the next section, treating "of the remote and proximate cause of gout," we are favoured with a recapitulation, and even a repetition of many of the excellent things, which had been several times so cleverly said in the former: an account of the remote causes, as generally conveyed by systematic writers, graced however by the florid and epithetic diction peculiar to our eloquent Doctor, is then introduced; and a display of the proximate cause follows, done so much in his best manner, that although incomprehensible by our Boerhaavian brains, we cannot resist the temptation to insert it.

"The proximate cause of gout results from the aggregate efficiency of the remote causes, and is truly the disease itself. This efficiency, or proximate cause, by which the disease is constituted, consists in an agitated and an increased degree of vital or repulsive motion in the affected parts. By vital motion is meant a *repellency, subsisting between the constituent particles of all matter.* This innate power or property is, by a law of nature, spontaneously *evolved from atomical surfaces*, and assumes character and *determinal force when issuing from the congeries or combination of material substances*, which forms specific or particular structure. The exertion of this universally *repellent power* in the organic fabric of the animal economy is *life or vital motion.* The action of this power denotes itself in animal feeling, as heat; an *undistinguishable identity*, therefore, with respect to the object, subsists between what has been variously denominated *repulsive motion, vital action, and heat.* This view of the nature of vital power instructively develops and explains the real cause of all the distressing torture occurring in *inflammatory* gout, [gout then is not *always* inflammation] to consist in almost a *combustive degree of redundant heat, or repulsive motion.*" p. 76.

During the paroxysm of gout, it was supposed by Hippocrates, Cardan, Sennertus, Liger, Sydenham, Stukely, and others, who believed in arthritic acrimony, whether as a flatus, spirit, or aura, a sharp, salt subtle humour, or "a liquid phosphorus and subtle poisonous gas," that the peccant matter was thrown on the extremities by nature, *optima morborum medicatrix*, for the relief of the system. Dr. K. disposes of his cause of the disease in the same way. "Gouty excitement," says he, "is an agitated and consequently an excessive evolution of *vital motion, endeavouring to escape* by the affected part, *torturing the sentient fibre by its accumulated force.*" The idea of gout being a combustion or conflagration, occurs in a treatise published by Dr. Stukely in the year 1734, to recommend, as a safe and infallible cure for the gout, "a *warm* oily composition, prepared by Dr. Rogers, an eminent apothecary and licensed practioner at Stamford." "The matter of gout is a certain fiery drop like liquid phosphorus, a caustic animal salt, dissolved in a rectified spirit of animal juices, which being disagreeable to the animal economy, is thrust out by nature into the glands that separate the oil, in order to extinguish it there; like putting a piece of hot iron, or live charcoal, into a vessel of oil; it extinguishes the iron or coal, but sets the oil on fire, which causes all the symptoms of the gout. The pain and heat by degrees derive a vast flux of blood and humours, which distend all the circumjacent vessels, in order to quench the *incendium.*"

Two sections of this work still remain to be noticed, the first treating of the cure, the other of the prevention of the gout: the latter conveys many laudable precepts in favour of temperance, which have however been detailed with equal force and less hypothetical complexity by Dr. Cadogan; who it may be recollected, effected a radical cure of the disease by temperance, abstinence, and peace of mind; of the paroxysm, by milk, meekness, soft water, gentle laxatives, and bread poultices. In regard to the cure, it is admitted by Dr. Kinglake, that the cooling treatment, or rather the topical application of cold water, was recommended by Celsus, and pursued by Harvey and others: he is not therefore to be understood as laying claim to the honour of originality; but as he assures us, that "the treatment was deduced from theory," we must grant that the practice was new to him, when he undertook it for the purpose of supporting his theory. "The ownership," says he, "claimed by me, is its *scientific investigation;*" of which, if we can imagine the depth to which literary depredators will descend, we may pronounce the property perfectly secure. There is one claim to merit, which in settling the doctor's pretensions, ought not to be forgotten; those spell-like fascinations, which have commanded experience, and charmed into a belief that to adopt his opinions was to break through the bondage of authority, and shake off the degrading fetters of prejudice. Immediately several minor spirits discovering buoyancy and elevation unhoped for before, by corruscating in the train of this medical magician, appeared as contributing proselytes: they were *liberal, judicious, ingenious and enlightened cultivators of practical medicine; men to whom the best interests of the healing art might be safely confided; whose communications discovered much scientific liberality and public benevolence; evincing ingenuousness and love of truth; proceeding with a correctness of* *of investigation honourable to science, and with a warmth of benevolence honourable to their feelings;* (Vide Annotations passim.) In short, through the medium of those delicious annotations to the cases published in the appendix, such a stimulant dose of flattery was exhibited, that in the short space of three years, a ponderous volume of cases and annotations (in which abundant provocatives for another are supplied) has been evacuated from the Taunton press. In the business of compliment, there must be reciprocity; and whilst the impertinent scurrility of the Hunts and Edlins, and the whole tribe of reviewers, rages with its utmost malignity, the Doctor may exclaim with Falstaff, "Let there come a tempest of provocation, I will shelter here."

In discussing the merits of the practice recommended and pursued by Dr. Kinglake, we must be directed entirely by the best experience which we are able to collect. *A priori* it is obvious, we should have decided that when a temperature exceedingly elevated prevails, nothing could be more properly or successfully resorted to, than fluid media of temperatures considerably inferior. No energy of prejudice could have resisted this conclusion, if powerful facts had not occurred to oppose its application. Were we to enter into the merits of the theory of our author, if we may be allowed to abuse the term *merits*, by using it where defects only are to be found, we might perhaps succeed in shewing, that Dr. K. has committed the *trivial oversight* of taking the effect for the cause; and that whilst he conceived himself to be engaged in subduing the cause, he was merely alleviating a symptom. This alleviation we ought not to despise, because we know that the effects of diseased action may become sources of new derangement, or that there may be *symptomata symptomatum.* But if preternatural heat, the effect should so far absorb our attention, as to withdraw it from the use of means which have a more radical influence, or if the alleviation should be procured with the risk of inducing danger more serious than the disease itself, the simple dictates of prudence would compel us to forbear. The cases published by Dr. K. have, we acknowledge, allayed the fervour of our dread of cold applications, but they do not annihilate the evidence adduced by many enlightened practitioners, that gout may be repelled, or that gouty action in an extremity may be transferred to parts where it would have the power to produce much greater mischief. We cannot confide in an indiscriminate declaration of safety, and therefore we regret that criteria have not been discovered, distinguishing the cases in which it would be safe, from those in which it would be dangerous to attempt the alleviation. In this respect, as in many others, the procedure of Dr. Currie, might, with the utmost benefit have been imitated. In the elegant and classical treatise on the affusion of cold water in fever, which may indeed lay claim to the title of "a scientific investigation,"-and have that claim allowed, precise and perspicuous boundaries are described, within which the mischief producible by the misapplication of a practice generally salutary, will not be found. Had equal urbanity, candour, and ability regulated the inquiry into the operation of the same power in gouty affections, its progress would have been much accelerated; and an active interest in deciding the question would have appeared among the higher branches of the profession.

The lofty, unmodified pretensions of Dr. Kinglake, will not we suppose descend from their towering elevation to admit the necessity for distinctions such as we demand; his panacea, like the boasted implements of public and avowed empiricism, must be universal. In many hundred instances, nay thousands, we are told, it has manifested no destructive power, and "the utmost vigilance has been unable to adduce a single *unequivocal* testimony to its discredit, and but very few indeed that are even pretended to afford evidence against its safety," (additional cases p. 408.) Dr. K. has not surely yet to learn that the want of published unsuccessful cases in the early stages of a practical inquiry, does not amount to positive proof that none have occurred: this proof of excellence once attached to the promising, but now almost forgotten aërial performances of Beddoes, the sanative grimaces of Mesmer, the metallic talisman of Perkins, and the wonderful acidulous gout tincture of Welles. Dr. K. ought not to forget that he has interfered with the regular progress of evidence, by declamations against prejudice, illiberality, and ignorance, from which in his earliest communications, he decided that all opposition must necessarily flow. Dr. K. will farther recollect that the constitution of the medical order, does not compel any man to criminate himself by stepping forward to proclaim his professional murders, and announcing, for the certain promotion of his interest, that he had consigned to the grave a patient or a friend whose existence had been committed to his care. Success it ought to be remembered is acknowledged with more satisfaction than defeat; and it is more comfortable to take credit with the public for the preservation, than the destruction of life.

We cannot allow to Dr. K. that no case unequivocally hostile to the practice, has occurred. The late Mr. Edlin, surgeon in Uxbridge, produced one which is decisive so far as medical evidence can be decisive, and which no sophistical cavilling, or splenetic abuse can ever invalidate. (Vide Edlin's two cases of gout.) It is the case of Mr. Baker, also a surgeon of eminence, who in the first attack of gout, applied to his inflamed feet ice and cold water: this practice was followed by most serious injury to the vital functions, which even Dr. K. in his reply tacitly admits, and he is driven to the necessity of maintaining without the shadow of a reason, that death which ensued, was occasioned by the medicines employed. These symptoms were *not* owing to gout driven to the contents of the thorax or abdomen, "for neither of them (referring to Dr. Haworth and Mr. Edlin,) with all their flippancy of decision, ever saw a case of repelled gout;" (reply to Edlin p. 27 ;) "the symptoms were those of *systematic commotion from sympathetic influence*" or "*visceral participation in gouty excitement.*" (Dissertation p. 25 and 26.) But though this may not be termed *misplaced gout*, it must be confessed to be *misplaced sympathy*: it was surely *superfluous*, painfully to sympathize with a part from which all pain had departed, and *absurd*, to begin just at the time when the call for sympathy had ceased: but it was probably reverse or perverse sympathy. We infer, let the Doctor disguise it as he will, that the morbid excitement was transferred from a part, in which little evil could arise had it remained, to one where it was capable of exerting a fatal power. In charging the medical attendants with the murder of their patient, the Doctor we fear has inadvertently confessed a design to commit murder himself. Mr. Edlin administered a draught consisting of mistura camphorata, ether, and confectio opiata, when Mr. Baker was found lying on his back, with a difficult hurried respiration, his extremities cold, his pulse quick, fluttering and intermitting, complaining of a palpitation of the heart, and an icy coldness of the stomach. Now Dr. K. in a case which occurred subsequently to this condemnation of the practice, ordered two tea spoonfuls of a medicine composed of equal parts of tinctura opii camphorata and ether "to be taken every four or six hours according to the urgency of coldness of stomach or that of spasm, if either any cold sensation at the stomach should sympathetically arise from durable chilliness (occasionally induced on the skin by the long application of cold water,) or if spasmodic twitches should recur." We lament that Mr. Edlin, whose conduct in this affair appears to have been exceedingly honourable, should have insinuated that it was possible that Dr. K.'s cases "might be fictitious and composed for the purpose of explaining a particular theory:" such insinuations by whomsoever made ought to be reprobated, and as the *little* cases alluded to make so slightly for the cause, if manufactured for the purpose, they must have been the work of a pitiful artist. The cases without any of the simplicity have all the *simpleness* of truth.

In the treatise of Mr. Parkinson, entitled Observations on the Nature and Cure of Gout, are likewise several instances strongly marking the injurious influence of diminished temperature in this disease. This ingenious and respectable author has with considerable success attempted to connect the valuable discoveries and analytical investigations of Scheele, Forbes, Pearson, Fourcroy, and Wollaston, with the origin and progress of gouty affections. To Mr. P. "the cause of gout appears to be a peculiar saline acrimony;" (observations p. 3;) and in the state of digestion, the cutaneous and urinary secretions, and the gouty concretions in particular, it is manifested to be an acid: the concretions being proved to consist of urate of soda, he concludes it to be the lithic or uric acid of late experimenters. This view of the cause naturally leads to the exhibition of alkalies of which soda appears to have been in Mr. P.'s own case and in that of others very benefical. Dr. K. has with his *usual* abilility and success attempted a refutation of this theory; and after publishing a large pamphlet in opposition to Mr. Parkinson, shews by a recent assertion, that he has never understood the subject. Mr. P. says he, "has imagined the gouty matter to consist of a peculiar saline acrimony affording *an acidifiable basis to uric acid*" (additional cases of gout, note to p. 4.) This we suspect is a mis-statement. Such a sentence, *totidem syllabis*, we allow may be found in "the Observations." We are not surprised that Dr. K. should deem a theory, which however defective, rests as far as it has foundation on uncontested experiments, to be derived from "speculative chemistry," since he appears to have kept aloof from all communication with the science during its best periods: nothing but the most culpable inattention or the most hopeless incapability could have left him so ignorant of a fact known to every *tyro* in the science, as the following quotation indicates.

"The rapidly evapporant quality of ether at the human animal temperature, renders it undoubtedly refrigerant, but certainly not in the degree in which water at the atmospheric temperature operates and which is requisite to the speedy and effectual extinction of gouty inflammation. The reason is obvious; ether is already surcharged with heat, or repulsive motion, and is consequently too fugitive to admit of any considerable addition of that property previously to evaporating; on the contrary the comparatively low temperature of cold water, will copiously receive and proportionately transfer inflammatory heat from the affected part." (Dissertation on Gout, Annot. p. 257.)

We shall not insult our readers by inserting the *little* experiments by which Dr. K. might be instructed. The same ignorance combined with credulity accounts for the note introduced to explain the "crackling noise heard issuing from Moses Simon's foot," when the said Moses "kept his foot for ten minutes under a large cock from which cold water was *poured with great force;*" (letter from Mr. Mullet, App. p. 216;) on which the Dr. sagaciously asks, "was not this sound occasioned by the *vibratory percussion of repulsive motion or heat* in its rapid evolution and escape from the inflamed part, through the comparatively low temperature of cold water?" (Annot. p. 217.) The same occurrence we may mention, by the by, was noticed when *moxa* a *burning remedy* was applied, and was then explained by the escape of *flatus*, the gouty cause of former days like heat in the present.

If this article had not already extended beyond the limits at first proposed, we should have directed the attention of our readers to the character and quality of the cases detailed in each volume. In regard to the first we feel it the less necessary, on account of the very able and complete exposition of their inadequacy contained in Mr. Hunt's "Salutary Cautions," and the last being very similar to the former both in kind and result, the objections need not be repeated. Hr. Hunt is severe, unnecessarily so we think, in his remarks on the suppressed cases, or what Dr. K. terms "neglected experience," consisting of "many less decisive instances of benefit." Now we are not inclined to believe that these were cases adverse to his plan, because he had an opportunity of applying his annotations (without which he never trusts any case out of doors,) to every objectionable part, and the answer always appears so satisfactory and conclusive—to the Doctor—that cases so fortified, could not bring him into any peril. When however we peruse the narrative of the old lady at Jedburgh, who for some crampy feelings dignified with the name of gout, put her foot into cold water instead of standing on the marble hearth or turning her shoe wrong side upwards on going to bed, as other wise old ladies often do, and of the consort of George the Second, who to render the exercise of *walking* more tolerable, "*oftener than once,*" during the *paroxysm of gout* dipped the affected limb in cold water, we are really at a loss to comprehend the insignificance of those cases which were not worth publishing. But these two *strong and striking* cases, with a quantity of common-place declamation against prejudice and illiberality, and a tolerable portion of adulation applied to the Doctor, receive as part of the regular annotation, the following character.—"This very intelligent communication is replete with both scientific and *practical* worth. Its judicious author views the subject with true philosophical criticism and dignified liberality."

In order to prove that no experience however despicable, has been neglected in selecting the additional cases, and to shew how easy it is, were it worth having, to gain the Doctor's praise, we insert the following communication from a *physician* in Glasgow; a genuine *Scotch* graduate. *He* must be astonished and those who know him, if we are not strangely misinformed, must be still more astonished, to find that he has suddenly become *ingenious, intelligent, and philosophical;* yet such is the encomiastic language awarded by the prurient gratitude of Dr. Kinglake.

"In corroboration of your practice, I shall mention an anecdote of a Mr. R. M——of this place, who was considered as an eccentric genius; he was a considerable grocer, and among other articles dealt in leeches; he had attacks of the gout in his feet, and the method he took for relief was putting them into a tub of cold water, where the leeches were, allowing as many of them to fasten on him as *chosed*, and he always found relief. This is a tale of forty years old; I know the man, and have often heard of his case, *but cannot vouch more for it than I never heard it contradicted.*"

"I can say of myself, that I have been occasionally distressed with a burning heat in the soles of my feet, which has obliged me to get out of bed and put them into cold water; I always found relief, and never felt any bad consequence."

Ex uno disce omnes. We intended to have entered into a minute examination of the cases contained in Dr. K.'s last publication, but after perusing them we thought very little advantage could arise from their analysis. They are in general, instances in which the mild antiphlogistic plan, now commonly pursued where the inflammation is active which is always the case in constitutions of vigour, would have brought the disease to a speedy termination: in this plan it will be understood that we include gentle saline laxatives, and mild unirritating diaphoretics. Sydenham was accustomed to consider the gout curable in seven or eight days: by *milk, and by opium* it has been said to be removed in three; and the warm oil of Dr. Rogers and the tincture of Mr. Welles, we are assured on *very good authority*, have succeeded in less than forty-eight hours. Recollecting the spirit of liberal inquiry which abounds among the medical practitioners in the metropolis, we have been surprised that this *choice and valuable* collection should contain only two, acknowledged to be treated in it, by Dr. K.'s plan. The narrators of these successful cases do not stand *very* high in their profession, and although abounding no doubt in eloquence and patients, they have not enjoyed sufficient influence or opportunity, to extend its beneficent power beyond the limited surfaces of their own persons

After all the labour and attention which we have bestowed upon this profound metaphysico-medical inquiry, we are compelled to confine our recommendation of Dr. Kinglake's favourite part of the cooling treatment of the gout, to its occasional moderate use in highly inflammatory cases. It may then we believe, be found an useful appendage to the sure, safe, and constitutional antiphlogistic means recommended by Sydenham, Cullen, Heberden, Darwin, and others, and now extensively adopted. With respect to the wonder-working powers of cold water when applied for the cure of gout, we must contentedly remain among those, to whom Dr. Kinglake may say in the language of Prospero,

> "You do yet taste
> Some subtleties o'th isle that will not let you
> Believe *things certain.*"

Art. VI. *An Attempt at a Systematic Reform of the Modern Practice of Adhesion; especially in relation to the Use and Abuse of the Thread Suture: with a View of its Merits comparatively with those of the Adhesive Strap in the Surgery of Wounds: being the substance of a Course of Lectures intended to have been addressed to Students in Surgery.* By Samuel Young, of the London College of Surgeons, &c. &c. 4to. pp. 158. London: Matthews and Leigh, 1808.

MR. JOHN BELL, in that part of his principles of surgery where he delivers "rules for the reunion of wounds," declares that "except in those cuts which are so slight as only to require a cloth to be wrapped about the part, every wound ought to be sewed." From this seed the present volume has grown. Mr. Young did not expect such a precept from such a source, and has published this book, in order to prevent the evils which were likely to arise from a rule so pernicious, supported by an authority so impressive: thus far he was right. It is clear that Mr. Bell has stated an opinion much too indiscriminate, and it became necessary that some one should point out the error which this eminent writer had committed; to show that although in his voluminous works there were deep waters through which we might safely pass, there were frothy shallows on which we should be in danger of striking. The task however required little effort. It was only necessary to describe the modes in which wounds are healed by nature, to point out the helps and the impediments which are in the hands of the surgeon, to shew that although sutures are favourable by producing contact, they are unfavourable by exciting irritation, and that they are applicable only in those cases, where the benefits arising from one source more than compensate the evils arising from the other. The more concise, distinct and simple the statement, the better would this task be executed. There was little to say, and that little was old. Mr. Young might have unburdened himself, and satisfied the public in a small pamphlet; but alas! such confines would not hold the ambitious soul of our author. He had found an excuse for appearing before the public, and he was determined to produce an impression; he was about to ascend the rostrum of instruction, and he resolved to mount in style; he was going to play the part of a teacher, and he cast his eyes around on the great men of the world, to observe how they acted, and how they talked, and thus enable himself to mimic them closely.

Authorities were not difficult to be found. A few years back the celebrated Bishop of Landaff not having an opportunity for delivering his opinions before the house of Lords, published what he had wished to say, and entitled it "*A Speech intended to be spoken.*" Here was a precedent too tempting to be neglected. Our author entitled his book "*Lectures intended to be delivered.*" But the work of imitation was still far from complete, and our author in his next search certainly displayed a sly dexterity, for which we applaud him abundantly. Who would have suspected that the man whose precepts he was contradicting, whose inconsistencies he was exposing, and whose ears he made to ring again with the furious blows of his rhetorical staff, should at the same time be secretly the object of his imitation? Yet nothing is more true. In pursuing the bird he had stolen its plumes; in hunting down the mad dog, he has been bitten himself: Hence we may observe in the styles of Mr. Bell and Mr. Young, the same habit of rambling from the subject in search of decorations, the same attempt at picturesque description, the same love of oratorical flourish; in short, the same marks of an abundance of fancy disproportionate to the intellect of the writer. Mr. Young however in unpluming Mr. Bell, has left the best feather behind. He has fallen far short of his model in that power of exciting the reader's interest and zeal in the study of the profession, for which Mr. Bell is at once distinguished and admired.

Hitherto our author had displayed in his imitative career, a degree of skill and dexterity for which we give him all due credit. A title and a style were both very necessary, and we think he was perfectly right to steal rather than to make them; but having proceeded thus far, he became so thoroughly captivated by his newly found faculties, as absolutely to go out of his way to find excuses for their exercise. The theory of inflammation which occupies no inconsiderable space, and which our author unfolds with a formality indicating that it is an essential preliminary to what follows, has no necessary or obvious connexion with any other part of the volume; when he comes to institute practical rules of his own, or to contradict the practical rules of others, he totally forgets his speculative doctrines, and no one can perceive where the one bears upon the other. If the whole theory of inflammation were cut out, the reader would miss nothing but the numbers of the pages. The fact is, as we believe, that he was determined to have an opportunity of imitating no less a person than our old friend John Brown. Here he states the very novel principles, how the power of life accumulates during repose,—how it is consumed by stimulants,—how it is exhausted during action, and how the power of stimulants is diminished by their repeated application, and increased by their temporary subtraction; then he goes on to talk about hungry stomachs, frost-bitten toes, and that strange tingling of the skin which we all feel on going from the cold air of a street into the warm atmosphere of a parlour, explaining them all with admirable fertility.

We somewhat suspect that our author's list of plagiarisms does not finish here, but we submit the conjecture which we are about to make with all due diffidence to our readers. The unknown author of Junius often suffers a certain grammatical error to slip from his pen, which has been used by some critics for the purpose of detecting him. When a plural substantive is interposed between a singular noun and the verb with which it ought to agree, Junius makes the verb plural. Now it is curious that this peculiarity of style may be found, *parvis componere magna*, both in the writings of Junius, and of Mr. Young, which we can hardly persuade ourselves was an accidental coincidence. Thus in page 47, our author writes "when the *adhesion* of parts *go* on healthfully," &c. If we are right therefore in suspecting that in this instance our author has exercised his much loved faculty for imitation, we may safely say that he has borrowed his title from Watson, his style from Bell, his theory from Brown, and his grammar from Junius!

We dare say if we were to search for more instances of imitation, we should find them in abundance, but we dislike the task of seeking out faults, and have doubtless already given our readers a satisfactory proof how alert and sharp-sighted we can be in the discharge of our critical duty. We must now endeavour to give some account of the subject matter of this curious volume, which is written in so eloquent a style, that it is not very easy to lay hold of the argument. The following contains as distinct a statement of what Mr. Young has to say upon the question, as we have been able to find.

"Now then to proceed to the Practice of the Suture: A man gets a longitudinal cut of the thigh of six inches; if you follow the exact rule, you place five sutures, that is, a suture for every inch: or if so many are thought useless, say three only are placed. Will this be sufficient to procure the adhesion of the sides of the wound? no: adhesive straps are to be placed intermediately to bring the remainder in fair contact. But how is it that three sutures only are placed? why are the intermediate spaces left to be brought in contact by the straps? why not apply the five; or why indeed use the interrupted suture at all? if the suture is so necessary for the procuring of adhesion, why not carefully sew up the wound altogether? the reason is, that experience has shewn that the wound will not bear such *tight lacing*: it inflames, swells, and if the surgeon will not cut out the stiches, nature, at the risk of gangrene and death, tears them out herself! can any lesson be more evident? at the very outset it is demonstrated that the thread suture is radically defective; as a foreign body entirely ruinous to adhesion; no other in fact than a seton. Experience has of late, rendered the practice of sewing wounds very fidgetty, full of contradictions and shuffling. A surgeon courts adhesion by the thread suture, as a specious knave advises his friend to adopt measures for his good, which a thousand to one will end in his ruin. Mr. John Bell cautions you against sutures as *necessary evils*: while they remain in a wound you must always be on the alert, ready in an instant to cut them out; and his manner evidently shews how happy you ought to feel yourselves if you escape their mischiefs. Thus you see, (without the most distant thought of using a pun on so serious a reflection,) all the boasted advantages of adhesion—the whole fate of that process which ranks among our first discoveries, hangs upon a thread.

"I think you would now consider it an idle waste of time to say more of the thread suture as an extraneous or foreign body, we will therefore proceed to consider its *mechanical properties* as answering the purposes of adhesion. The *fair* and *equable contact* of the sides of a wound from bottom to top is necessary in order to obtain their adhesive re-union: how is this performed by the thread suture? can it be imagined for an instant, that a narrow cutting ligature of a fourth, a third, or half of a line in breadth, can effect a general uniform compressure? does it not amount to a demonstration from their very figure, that such sutures cannot possibly be placed in any form whatever to answer the purposes of a compress? is it not as evident that, wherever they are applied, *partial stricture* must be the consequence? in fact, that they must act as so many *fixed points* from which the rest of the wound must necessarily be dragging!

" Taking an individual view of the thread suture as answering the ends of adhesion, we find then, that among its very first qualities, it answers to an extraneous or foreign body; that from its very figure, it cannot possibly be applied as a compress; but on the contrary, that its application must always produce *partial stricture*: admirable qualifications to meet the ends proposed!

" The thread suture then is destructive to adhesion, as it is an extraneous body; *indirectly* it fails to produce equable compressure; and *directly* it produces partial stricture!"

Now this is what we call making a clear question very muddy. Every surgeon knows that the thread suture acts as an extraneous body; that where it touches it produces ulceration; and that a sewed cut would be ill secured without the additional support of plaster and bandage; but we must not infer, what in fact is false, that there are no wounds which require the aid of the suture. To say that all cuts ought to be sewed, excepting those which would do as well under the Riga drops of an old woman, as with the plaster of the surgeon, is certainly saying much more than is true: some will require thread, and others will do without it, and perhaps no two surgeons will exactly agree as to where the line ought to be drawn; but we believe few will be found, who with Mr. Bell would use it *in all cases*, or with Mr. Young in *none*. It is for men of genius like them to post themselves at the extreme points; but in this, as in most other cases of the same kind, the medium is the residence of sober sense and truth. Our author continually fancies himself in the lecture room, and calls loudly on his hearers to remember " *what adhesion is*," and to acknowledge that the use of the suture is directly contrary to the principles which govern it. So picturesque a writer is our author, that we often fancied we could hear the call, but could never fancy that we saw the reason for it.

It is very obvious that a wound sewed by thread is irritated by an extraneous body; but nevertheless there are many cases where we are compelled to use a suture upon this plain principle, that the evil arising from irritation is over-balanced by the good arising from security. To Mr. Young's observation " that if sewing be useful, why not sew up the wound completely?" no reply is necessary; the question is, whether a suture, with plaster and bandage, does not approximate the edges of a wound more effectually than plaster and bandage alone? and whether there are not some wounds which require this additional security? After removing, for example, a large tumour from the axilla, or a scirrhus from the throat, or a cancer from the hip, plaster and bandage would not be sufficient to retain the edges in contiguity. Most surgeons would take a few stitches to assist the plaster in resisting the retraction of the parts; and in these cases most surgeons would act rightly. That a stitch affords additional power of placing and retaining the parts together, there can be little doubt. Whilst you are applying the strip of plaster, the blood oozes out and impedes the exact contiguity of the edges; if the wound is in the lip, the saliva is another and a more annoying impediment. When the wound is dressed up, the plaster and suture prevent the retraction of the parts more effectually than either could alone. The bandage may get loose, the blood, or the pus, or the saliva, may insinuate itself between the skin and the plaster, which by these means may lose its firm hold. Against all these possible and common dangers, the ligature is a guard; and when you have to secure a large flap of integument, as after an operation on the breast, it is absolutely indispensable. We have already detained our readers sufficiently long on this subject: we have only to request Mr. Young dispassionately to put this question to himself, Are not the sides of a wound preserved in a more perfect contact by ligature and plaster than by plaster alone?

We cannot take leave of this volume without informing our readers that they will find it copiously stored, not only with surgical and physiological materials, but with poetical graces. We think that in this line of excellence, our author's fort is personification; the adhesive inflammation passes from one shape to another as rapidly as if it were touched by the wand of a magician: the number of transformations which it undergoes is incredible; it is a man rescued from barbarism, a towering giant, a persecuted sage, a covenant, a revelation, a revolutionist, and the branch of a tree!! It shines on us as the sun, and we make love to it in the shape of a damsel. In short, it assumes such a Proteus-like variety of forms, that we were often for a moment puzzled to recognise the identity of our old friend.

Art. VII. *Observations on Aneurism, and some Diseases of the Arterial System.* By George Freer, Fellow of the Royal College of Surgeons, London, &c. 4to. pp. 116. London: Cadell and Davies. 1807.

We are of opinion, that no less weighty consideration than that of communicating new and valuable matter, should be admitted in defence of high-priced books, on subjects of pressing and universal importance. They may be tolerated in matters of speculative interest with which it is fashionable to be acquainted, but not in those of practical necessity, of which it is criminal to be ignorant. We are advocates for periodical journals, because if properly conducted, they give an easy access and a rapid diffusion to intelligence of this description: they are the telegraphs of science, and their contents are the signals, by an observation of which the interior and the outposts act in concert. Mr. Freer's case of femoral aneurism might have been published in the Edinburgh Journal more than a twelvemonth ago; it now costs a guinea, it would then have cost three shillings; it is now read by hundreds, it would then have been read by thousands: but it was a new result, an almost unlooked for circumstance, and by his friends' advice and possibly his own ambition, Mr. Freer was persuaded (and his deadliest enemy would have joined in the persuasion) to write a book! We acknowledge the perplexity of the question to an author, In what way will my story tell best, and leave the deepest impression upon the public? certainly not by diluting its essence with a maukish mixture of heterogeneous materials; not, after the prevailing fashion of modern bookwrights, by spreading a little matter over a large surface, and supplying the gaps of invention with scraps and citations. The effort is not only unnatural in itself, but pernicious in its tendency. A gentleman imposes upon himself a task to which his habits are averse, and too often his education unequal, and for which he is no better qualified than any other man who performs a similar operation with similar success. The world is kept in ignorance of an important fact, while he is straining his wits to fabricate a theory, and neglecting his business to compile a volume, which after all, are but specimens of crude conjecture and hungry compilation, and give no additional value to the case. To make an argument for a case, is however a very venial fault, compared with that of making the case for the argument; and, however we may be disposed to question the author's judgment in his plan of publication, we desire it to be understood that we have the strongest confidence in his statements as a practical writer. We have made these observations, because we set a high value upon the record of an operation which entitles Mr. Freer to great credit as an anatomical surgeon, and because we hail it as an auspicious proof that the "capability," as Mr. Tomlinson writes, "of performing the most complex and difficult operations, is not confined to the metropolis, or other great schools of medicine."

The observations on aneurism are very properly prefaced by some remarks on the structure and pathology of arteries, and on the power which these vessels possess, tending to the spontaneous suppression of hemorrhage. The opinions of Messrs. Petit, Pouteau, Kirkland, and John Bell, which attribute this effect to a single cause, are cited, and the experiments and reasonings of Dr. Jones that demonstrate the fallacy of these opinions individually, and the propriety of referring it to a combination of causes, are extracted from his admirable treatise in the Doctor's words. The observations of these experimentalists are correct as far as they go, and the reasonings from them are defective only because they went no further. The *caillot* of Petit, the *crispation* of Morand, the *engorgement du tissu cellulaire* of Pouteau, and the contraction of Kirkland and White, all exist, and co-operate to the same end. It is true they do not hold an equal rank in order of importance: the clot so accurately described by the first observer, and which Pouteau tried in vain to discover, is the first and most powerful of nature's agents; and the injection of the surrounding cellular membrane, upon which Mr. Bell so eloquently insists, is in fact the most insignificant in the suppression of hemorrhage. We are roused by the mention of this gentleman's name, to express our disapprobation of his disingenuous and ungracious adoption of the theory of Pouteau without acknowledgment. Where is the difference between the "bold speculator*" and the dashing assertor? "It was Mr. Pouteau's opinion," says Mr. Bell, "that *the swelling of the surrounding cellular substance compressed the artery, and stopped the blood*†." "When hemorrhage stops of its own accord, it is neither from the retraction of the artery, nor the constriction of its fibres, nor the formation of clots, but *by the cellular substance which surrounds the artery being injected with blood*‡." Where was the necessity of proclaiming and enforcing with all the pomp and parade of an original opinion, one that was proclaimed and enforced more than fifty years ago §? Did it arise out of Pouteau's malpractice of including the nerve in the ligature, which does not in the most remote degree affect the principle, or from Mr. B.'s opinion, that in the author of the stupendous "Principles of Surgery," it was *infra dignitatem* to espouse what it was deemed not inexpedient to borrow. It was by a most unhappy fatality in the order of events, that the above-mentioned volume made its appearance before the philosophically dispassionate and profound inquiry of Dr. Jones. It is there demonstrated, that the retraction and contraction of the artery are powerful adjutants to the adhesive process, and that the pressure from the infiltration of the surrounding substance, is never sufficiently powerful to form an effectual obstacle without them.

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* Princ. of Surg. vol. i. p. 175. † Ibid. ‡ Ibid, p. 179.
§ Mélanges de Chirurgie. A Lyon, 1760.

The object of Mr. Bell's most unqualified and outrageous assault is the celebrated M. Petit, an author so peculiarly fated to misrepresentation, as almost to convince us that his real opinions were too correctly formed to admit of aggression without libel or perversion. From an inaccuracy too gross to admit of excuse or palliation, the experiments and notions of Petit the physician, (Sur les moyens dont on s'est servi, & dont on se sert presentement, pour arrêter les hemorrhagies causées par l'ouverture des veines & des arteres dans les playes. Par M. Petit, *le Medecin*, Mem. de l'Acad. Roy des Sciences. An. 1732.) who wrote in envy of the surgeon's high and deserved reputation, and who appears to have shared with him only the honour of his name, are held up to the derision of Mr. Bell's readers, as the veritable productions of the latter*. We know Mr. Bell's aversion to "*cruel remarks*," his horror of "*posthumous assassination*," his—but this is no laughing matter. Whether the gibes and sneers in which Mr. B. so delights to indulge, are as decorous upon this occasion as he appears to think them clever, we do not stop to inquire; but we feel it our duty in justice to declare, that in their application *they are utterly false*. We would by no means insinuate so ill of any man's moral character, as to suppose him guilty of wilful and malignant aspersion; wherefore we call upon Mr. Bell to make reparation for his misdemeanor, by doing that justice to the memory of the illustrious Petit, which however ample must be scanty, and however early must be slow.

But to return to Mr. Freer. "Desirous of trying experiments," Mr. F. obstructed the canal of the radial artery in two horses † by the pressure of a tourniquet, and the result was a deposition of coagulable lymph, not in the cavity of the vessel, but surrounding it; and an effusion between the coats of the artery, whose tube was so much contracted and pressed together, as to render it completely impervious to blood. The following we offer as the explanation of these appearances. Inflammation from pressure is probably first in order, and greatest in degree in those parts (provided all are of equal integrity) which have nearest local relation to the compressing cause. Here the inflammation begins from without, and the sides of the artery are therefore compressed, not united, by lymph. But if any of the compressed parts have previously acquired an undue and morbid sensibility, the effect will be somewhat inverted both in order and degree. Thus the result of the experiment will be essentially different upon the sound and the inflamed artery. Pressure will likewise have its effect modified by the degree in which it is exerted. Thus the artery, in a spontaneously cured aneurism, presents an appearance similar to that produced by the ligature which divides the inner coat, and indeed we believe that this appearance uniformly prevails in recent permanently obstructed arteries. We have seen within these few days, the canal of the human carotid perfectly obliterated by a coagulum nearly three inches long, and adhering firmly to the sides of the vessel. Here the aneurismal diathesis was present; and the patient died from the bursting of the tumour, which had made a moderate but gradually increased pressure upon an artery, the internal coat of which was rendered susceptible by a primary morbid impression.

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* Princ. Surg. vol. i. p. 171.

† From the remarkable strength of the arterial coats in the horse, and the animal's insusceptibility of aneurism, we doubt if pathological inferences can fairly be drawn from this source.

Mr. Freer has often found the coats of the great arteries inflamed in dropsical bodies; in one case he has seen anasarca cured by copious draughts of cold water, and in another by the adoption of a most decidedly antiphlogistic regimen; hence he is induced to believe that *arterial inflammation is one of the causes of dropsy*. This speculation is *confirmed* by the accidental discovery of an ulcerated iliac artery in the body of a man who died of hydrothorax. Two brothers of this man laboured under similar diseases with himself, "*and it is not unlikely,*" says Mr. F. "*that in all three, had I had the power of accurate examination, but that the arteries would have been found in a state of general inflammation!*" &c: So perfectly does this most gratuitous assumption satisfy Mr. Freer, that he adds, "Would it not *therefore* be proper in these cases to try bleeding, diluents, and an antiphlogistic regimen, instead of the drastic purges and stimulant diuretics commonly recommended?" How far does the experience of Morgagni confirm the notion of Mr. Freer? He found the arteries of some dropsical bodies inflamed, but in proportion as the effusion was considerable, the inflammation was slight, and vice versa. This approaches nearest to the result of our observation, which teaches us that secretion and exhalation are uniformly diminished, to a certain extent, if not totally suppressed by the inflammation of the secreting or exhaling organs. But, however the fact may stand, we submit it to the sober consideration of our author, whether a link or two is not wanting to establish the necessary connexion between inflamed arteries and anasarcous effusion, of cause and effect? From the symptoms of this patient, we cannot help regretting the want of information respecting the heart and its valves. There can be little doubt that this organ was the original seat of disease.

The spongoid tumour of Burns, and the fungus hœmatodes of Hey, Mr. F. classes among the diseases of the arterial system, and defines to be "*an aneurism of the extreme arteries.*" Mr. Pott's cases of disease and rupture of the posterior tibial artery is first quoted, with what propriety our readers may judge, since it palpably subverts the hypothesis. Then follow the minute description of Mr. Burns, and three well related cases from the wards of the Birmingham hospital. We regret that Mr. F. lost the opportunity of examining the body of Jones. We remember a case much resembling his, in which, upon inspecting the body of the patient who fell a victim to the disease, the surface of the right lung exhibited an ulcerated surface of the breadth of a shilling.

If these cases have any value, it is for the fact ascertained in the examination; viz. that the arterial trunks and larger branches were in all perfectly sound.

We have now no means of ascertaining if Mr. Pott asserted from fact or apprehension, that the artery in such cases would be found "enlarged, distempered, and burst." For ourselves we rather incline to believe, that this was a *façon de parler*, a figurative mode of expression, in which Mr. Pott occasionally indulged. But because the trunks and larger branches present no signs of disease, we really cannot discover the necessity for attributing it to the capillaries: Mr. Freer, however, seems to have contented himself with reasoning thus upon the negative. The *large* arteries are free from aneurism, hence it must be an aneurism of the *small* ones. But what single character, we ask, does the tumour possess, assimilating it with aneurism? It is hardly consistent with our knowledge of that disease, to suppose that it should attack at once all the arterial extremities of a limb; and yet this it must do to explain the greater extent of the fungus hæmatodes than that of a diffused ecchymosis. After so freely stating our opinion of the author's hypothesis; it may seem hazardous to venture a manufacture of our own: but no man is a patentee in theory; the market is open to articles of every description, and as we substitute ours for what we think a coarser material, we will cheerfully withdraw it for one of better quality.

The extreme terminations of arteries, it is well known, have a power a power of self extension or elongation, upon which depends the organization of lymph deposited, and consequently the reparation of parts destroyed. The interstitial structure of the body, soft and hard, presents a cellular arrangement, and the reticulum of the cell, whether of bone or membrane, is in part constituted of nutrient and secerning vessels. From their orifices they exhale the separated fluid, whether adipose or medullary, albuminous or earthy. Inflammation seizes the cell; and the secreting vessel in lieu of its natural fluid, effuses lymph, if of the cancelli, the disease commences in the bone; if of the soft cellular tissue, it forms its bed among the muscles. The process of deposition is followed, according to an invariable law of nature, by that of nutrition; and to keep pace with the former process, the vessels destined for the latter become elongated into its substance. As no pressure exists to modify the extent of the deposit, the extent of the vascular growth is likewise unlimited; and thus the processes alternate with a celerity characteristic of disease, which is too great to admit of the effusion of perfect lymph, or of perfect organization. When the disease has begun from within, it advances slowly, because it has to destroy the encompassing laminæ of the bone, periosteum, and muscles; and the investing fascia of the limb, where it is of great strength, as in the thigh, will sometimes so far resist its progress as to produce a diffused and uniform enlargement from the hip to the knee. We lately saw a case of this description, in which the circumference of the thigh in a girl of seventeen measured 37 inches. The compression which some parts of the swelling undergo from the same cause, will give it a partial firmness or even induration to the touch. Its advance is more rapid, and its signs less equivocal, when it originates near the surface; and if it affects the superficial cellular membrane, it soon becomes an extensive and ill conditioned fungating ulcer.

Upon dissection, it uniformly exhibits a semiorganized mass irregularly intersected by septa, and extending in the direction of the cellular substance; the muscles are only secondarily affected. The surface is usually of spongy consistence, yielding to the slightest touch; and deeper it presents some bony spicula and loose coagula floating in grumous blood. The finest injection pervades some minute vessels of the interior, but extravasates, as it approaches the surface, into an indefinable mass. We have known it contained within the fascia lata, which in a section of the thigh resembled an entire cyst: it has no proper sacculus, resembling the encysted tumour. We therefore believe this disease to be a local inflammation of the arterial extremities of the secreting cell, whether general or specific we cannot yet determine*. Every disease is modified by the structure in which it exists, and probably the bloody fungus is in the simple or follicular gland, what soft cancer is in the visceral glandular system.

Our author notices, apparently for form sake, the aneurism of anastomosis and varicose aneurism, and records a case of each; but adds nothing to the full and particular descriptions of Mr. John Bell, and Dr. Hunter.

In attempting to determine in what a true aneurism consists, our author states his opinion, that "though there may occur cases of original dilatation without rupture of the muscular coat, as some related by Morgagni, Lieutaud, and Senac†, yet by far the greater number of aneurisms arise from the latter cause; and though he has never met with a case which could not be referred to some "violent impulsion which might injure the coats of the artery, and produce rupture," still he believes in a peculiar constitution of arteries, which constitutes predisposition. He also imagines that *the muscular coat is sometimes dilated alone.* "But in general," says he, "*it is ruptured and then forms a sac, bulging out from the ruptured portion of the coat, and gradually dilates all the other coats from the active pulsation of the artery itself.*" In the first place, we exeeedingly doubt if the muscular coat is susceptible of dilatation. Let Mr. Freer strip off the ligamentous coat in which the strength of the vessel consists, and inject air or water with moderate force into the canal of an artery, and he will not fail to observe many small sacculi or pouches of the internal or cuticular membrane, protruding like so many herniæ, between the circular fibres of the muscular coat. In the experiment of Nicholls‡, the internal coat is said to rupture, and the external to form aneurismous tumours. His distinction applies only to two, an internal and an external; the former the muscular, the latter the ligamentous. This therefore proves precisely the same thing under different circumstances, viz. the non-resistance of the muscular coat. If the artery be sound, as in his experiment, and the force of injection continued, the internal or membranous coat yields sooner, because the attachment of the external to the muscular coat prevents the protrusion of the internal, and hence the pressure being equal on all its sides, it is susceptible of less extension. The statement that the muscular coat is dilated *alone*, implies that the internal coat is ruptured; now if the experiment before mentioned be repeated after a wound or separation of the internal coat, we are convinced that the muscular coat is incapable of resistance, and consequently of dilatation *per se*. 2. The statement *that the muscular coat is generally ruptured*, conveys the idea that the internal coat remains sound; this last therefore is " the sac, bulging out from the ruptured portion of the muscular coat, and dilating *all* the other coats." Here we are wholly at a loss. Having disposed of the internal coat by dilatation, and of the muscular coat by rupture, how many are *all the other* coats? We suppose at least half a dozen! and congratulate Mr. Freer on an anatomical discovery, in which we verily believe he has not been anticipated in the university of Edinburgh. We do not know if in this explanation our author laboured to be brief, but sure we are that if he had desired to be obscure, he could not possibly have succeeded more to his wishes.

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* Does the F. H. spread by absorption! We knew it appear in one case in the groin, after having existed for some months in the heel, but our experience hitherto would lead us to decide in the negative.

† Are those visible pulsatile dilations of all orders of arteries, from the aorta down to the labial and temporal trunks, which are so frequently induced by over exertion, or the consequence of acute diseases, properly to be termed aneurisms?

‡ Vide Phil. Trans. An. 1728.

Dr. Blane's case of internal carotid aneurism is introduced, to shew that this disease may be formed in the brain. The following information we extract as a specimen of the work, rather than with the hope of instructing our readers.

" When this disease occurs in the extremities, it is always distinguishable by the continual pulsation within the tumour, and by the length of time necessary for its progress.

" Abscesses may be mistaken for aneurism, when the time of their formation and coming to a crisis is not watched. Scarcely ever can there be room to doubt, when due attention is paid to the order of symptoms, and of their occurrence. Aneurisms in their progress give great pain, but the pain is more relative to the distribution of cutaneous nerves, than confined to the aneurismal tumour; whereas the throbbing pain of abscess is confined to the tumour. The pain and redness which attend aneurisms, when the distention of parts has arrived near its utmost, may be compared to those of abscess; but at this period of the disease, the pulsation will be so great as to remove every doubt. The stopping of pulsation by compressing the artery between the heart and the tumour, is the chief criterion of aneurism of the extremities, and the terrible pulsation of the aneurism of the aorta makes itself too evident when this great vessel is the seat of disease. Of the other symptoms which attend aneurism, there are few, characteristic of this disease alone. Syncope and vertigo are very frequent symptoms. Langour and debility generally attend it, and sometimes life is consumed by atrophy, and the body wastes away before aneurism of the aorta comes to its termination, either by bursting, or by compression of the passages of the stomach or of the lungs."

Mr. F. advances nothing new in the medical treatment of the disease. In the latter stage of the disease, when placed beyond the reach of art, digitalis and opium have been found most effectual as palliatives.

The succeeding pages of the volume are filled by a collection of cases of carpal, humoral, axillary, and subclavian; crural, popliteal, femoral, and inguinal aneurism. In a second case of radial aneurism Mr. F. would adopt the practice of Tulpius, which consisted in obliterating the vessel by the pressure of a leaden plate and roller. Ruysch's case, which proves the inefficacy even of a thin ligature where a foreign substance is interposed, accurately confirms the principle established by Jones. The case of Mr. Maggill, (Edinburgh Medical Essays) of aneurism from bleeding, is one in which compression failed; and that of M. Foubert (Mem dè l' Acad. de Chirurg.) in which it entirely succeeded, is we believe, that from the examination of which Petit drew his interesting description of the clot which closes the orifice of a wounded artery. (Mem. de l'Acad. des Sciences, 1735.) A cure by ligature of a humoral aneurism, and a fatal termination of the disease in the axilla, are taken from the practice of the Birmingham Hospital. Our author was prevented from undertaking the operation in the latter "but is satisfied that in these cases much may be done by the art of surgery." We think so too, but consider the operation above the clavicle, as one of difficulty from the necessary straitness of the wound, owing to the depth and relative obliquity of the artery; and of no inconsiderable danger from the vessel being imbedded in a plexus of nerves. The accident of tying one of the nerves which overlaid the vessel, if we have been rightly informed, happened to a surgeon eminent for his improvements in this branch of surgery.

Passing to the consideration of the disease in the lower extremity, Mr. Warner's cases furnish Mr. Freer with the history of one posterior tibial, and three femoral aneurisms. The account of Mr. Hunter's operation is extracted from the Medical and Chirurgical Transactions, and from the same work the interesting case of Mr. Marshall who died from secondary hemorrhage, on the 12th day after the operation for an aneurism of the ham. Lastly we have the case of Donald, (Edinburgh Medical Commentaries) Mr. John Bell's surprising adventure in the vicinity of the leech catcher's bottom, and the minute particulars of a "*femoral aneurism cured by tying the external iliac artery.*"

A thick rind covers a sweet orange, and this memoir was a *morceau* too precious to be got at without much paring and decortication. Mr. Freer falls into the common, we will not say vulgar notion, that popliteal aneurism haunts postillions and coachmen. Two of Mr. Hunter's first patients followed these employments, and hence he was led to generalize rather prematurely. The disease is we believe to the full as frequent in sailors. We call two cases immediately to recollection, in both of which the operation was performed with entire success. Porters who carry burdens are perhaps as freqnently the subjects of it; of this likewise a case very lately occurred within our observation. Persons who are suddenly compelled to take much foot exercise, are also liable to the disease. Of this we knew one instance, and heard of more during the feverish feats of the volunteer infantry. The horseman's continued genuflexion, and at the same time inversion of the foot, deprives the hollow of the ham of any point of contact, and of course prevents local irritation. Besides it is obvious, if so large a class of men were so peculiarly exposed to the action of an exciting cause, that the prevailing frequency of the disease among them, would have made us as familiar with the postboy's aneurism as the chimney-sweeper's cancer. Mr. Hunter's second patient underwent the operation about eighteen years ago, and has since been the guard of a mail coach: a few months past, the same leg was amputated below knee for a foul ulcer of very extensive surface, the drain from which had already much reduced, and would shortly have exhausted his vital powers*.

The subject of the operation performed by Mr. Freer was a muscular blacksmith; his age twenty-seven years; his general health good; the operation was done on the 4th October. Pulse before the operation, ninety; second day at five in the afternoon, one hundred and twenty; which was the highest point it attained. Saturday, 19th October, the ligature came away, and from this time (says Mr. F.) I considered him safe, and the operation to have completely succeeded. For the minutiæ which are full and interesting, we refer our readers to the work. Within a twelve month, Mr. Tomlinson, the colleague of Mr. F. repeated the operation with success.

The credit of first performing the operation for inguinal aneurism is due to Mr. Abernethy†, but in a most essential point, viz. *the mode of securing the artery*, the practice of the gentlemen above-named differs from that recommended by him. Before the publication of those experiments the result of which justifies the use of the single ligature, Mr. Abernethy proposed the application of two ligatures and the division of the artery in the interspace; because (he observed,) "when the ligature is applied in the middle of a detached portion so as to cause the death of the part inclosed by it, there will be still a certain extent, both above and below the ligature, liable to ulcerate, particularly in unsound arteries [*]." Possibly this tendency may exist in diseàsed vessels, but the successful termination of these and similar cases contradicts the general validity of this doctrine. In by far the greater number of cases however, the practice of Mr. Abernethy has been successfully followed for the cure of aneurism; and in the second of his three operations for the disease in the groin, the extremities of the vessel which had been so treated were united, their sides perfectly closed, and a plug of coagulable lymph found in each [†]. In the third which was performed one week subsequently to that of Mr. Freer, and followed with equal success, the sàme conduct was adopted [‡]. But it being established that the single ligature is secure in practice, no question we think can arise upon the propriety of recommending its universal adoption. First, the passage of it separates the vessel as little as possible from its attachments, and precludes the necessity of stripping the artery to the extent of more than an inch of its fascial sheath, the most tedious and perplexing process of the operation. Second, the external coat of the vessel remaining sound, there can be no risk of hemorrhage from the escape of the ligature; an occurrence so formidable as to require for its prevention various ligatures of security, which impede the surgeon in the operation, and nature in the cure. Third, the state of tension in the undivided artery is decidedly more favourable to the progress of the ligature, than the relaxation of the retracted ends; and the danger of secondary hemorrhage is never diminished by the slow separation of the ligature. If we were disposed to use two ligatures, we should pass them as single and distinct threads at the distance of an inch and a half or two inches from each other, allowing the extremities of the artery divided in the middle of the interspace to preserve their natural attachments. The sheath and surrounding cellular substance would thus form a natural knot upon the ligature, and be in our opinion a much safer, simpler, and less irritating barrier than the *ligature d'atteinte*, and the revived cross stitch of Dionis.

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* We are disposed to attribute the sloughy state of this sore and its total indisposition to heal, to defective circulation from ossification of the enlarged anastomotics, and not to any defect arising from the nature of that circulation.

† Vid. Surg. and Phys. Essays, Part iii. p. 163.

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[*] Surg. and Phys. Ess. P. III. p. 172.

[†] Surg. Obs. Vol. I. p. 226.

[‡] Ed. Med. and Surg. Journal, Jan. 1807.

The remainder of this surreptitious volume is eked out by observations reflecting on the operation by ligature, and as they are so potent as to have induced Mr. F. to recommend the old and often salutary practice of compression in a new shape, we extract them for the benefit of our readers.

"Such sometimes is the result of the operation for aneurism when situated upon the extremities, and the benefit of tying a ligature upon the artery above the tumour. But it is a well known fact that this plan frequently fails, and when we examine the state of the arteries where aneurism has existed, it is often evident that the injury done to the part by the operation on an aneurismal artery, is very liable to produce that degree of inflammation denominated ulcerative, in which case the operation will not succeed, and the patient will be probably destroyed by the bleeding of the vessel. Pressure is another mode of cure which has frequently been tried, but in general the pressure has been made upon the tumour itself, thereby producing much pain and occasionally mortification of the part.

"The failure of those cases mentioned in the Medical and Chirurgical Transactions seems to have arisen from the previous state of the artery, and I am well convinced that in most spontaneous aneurisms, the arteries in general will be found inflamed. Ligatures applied under these circumstances, instead of producing an adhesion of the vessel which alone can save the patient, frequently produce ulcerative inflammation, hemorrhage and death, without giving that chance of a cure, which nature alone sometimes effects; for I am induced to believe that as many successful cases will be found upon record, where pressure either by the assistance of art, or by the spontaneous swelling of the part, has produced a cure as are related of the operation.

"Deposition of earthy or ossific matter on the coats of arteries is likewise another attendant upon aneurism, which very much increases the danger of the operation. In subjects advanced in life, ossification of the arteries is not uncommon; and when a ligature is drawn upon a vessel so diseased, it either snaps at the moment, or cannot be drawn sufficiently tight to intercept the stream of blood; or some small spicula of bony matter are pressed against the opposite side of the vessel to which it will not adhere; the part ulcerates and a secondary hemorrhage ensues.

"The operation therefore for aneurism by ligature, is not always a certain experiment. This danger does not arise from the uncertainty of the anastomosing vessels carrying on the circulation, for where the disease has existed some time, no apprehension on that ground should intimidate us; but the probability of the ligature ulcerating the artery and thereby producing a fatal hemorrhage, is a circumstance too frequent not to be dreaded."

The practice of compression originated with the celebrated Guattani, who published his treatise "de Externis Aneurysmatibus, &c." at Rome in 1772. After some fair remarks on the generally good effect of pressure upon the soft and yielding parts of the body, and some analogical inferences of its salutary tendency, we are favoured with a translation of Guattani's cases of Valera and Damianus, and two cases are added that happened within Mr. F.'s knowledge in which the cures were imperfect*, and the patient's sufferings extreme.

The pressure recommended by Mr. F. is to be made upon the artery, and not upon the sac; it is to be applied by the assistance of Senffio's instrument †, or in the following manner.

"First, place a bandage moderately tight from one extremity of the limb to the other, then place a pad upon the artery a few inches above the tumour, that you may have a greater probability of its being in a sound state; then with a common tourniquet surrounding the limb let the screw be fixed upon the pad, having previously secured the whole limb from the action of the instrument by a piece of board wider than the limb itself, by which means the artery only will be compressed when the screw is tightened; the tourniquet should then be twisted till the pulsation in the tumour ceases."

We heartily congratulate our professional brethren that the barbarous fashion of amputating every aneurismal limb is at length fairly out, whatever honours may be in reserve for its contemporary. The pressure here recommended, will be found upon examination to have no better claim to preservation. We will first examine it in its effects as ascertained by dissection.

"The irritation produced by this mode of pressure excites that degree of inflammation of the artery, which deposits *coagulable lymph in the coats of the vessel, thickens them, diminishes the cavity, and eventually obstructs the passage of the blood.*"

In a preceding page our author says;

"From these considerations I have been induced to examine a variety of authors, who have written upon aneurism, and to enquire into the causes that produced a favourable termination in many, when the operation was avoided. In all these cases the artery was doubtless obstructed, and in those in which the opportunity offered of examining the tumour, that obstruction was always produced *by coagulable lymph filling up the cavity of the artery, uniting its sides, and interrupting the passage of blood.*"

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* In the first case, at the expiration of twenty years, there remains "*a very large tumour*," and in the second at the end of two, "*a little lameness from the tumour and the contracted state of the limb.*"

† Vid. Platner's Surgery.

Whoever will compare these statements must observe, that the immediate effect of pressure by Mr. Freer's tourniquet differs essentially from that observed to be produced in all cases of favourable issue, where the operation was avoided. Hence his recommendation of the practice rests not upon the result of the experience either of himself or others, but upon the single fact of the closure of the canal in the horse by a surrounding effusion; a mode of obstruction according to his own account, altogether unlike that which is observed invariably to prevail after the cure of aneurism in the human subject. It is surely necessary to point out this important distinction between the appearance of an artery after equable and moderate pressure, and that produced by violent and partial pressure. As an exemplification of the first case, we will suppose by the bandage of Genga, of the second by Mr. Freer's screw tourniquet. If it be a fact that no artery is permanently obstructed, in the cylinder of which lymph has not been effused, then the proposed improvement is not only a speculative but a dangerous innovation; for it will encourage to perseverance in a line of treatment, which though it produces excessive local irritation must ultimately fail, to the postponement if not exclusion of another, less severe because less protracted, and founded upon the certain principle of effecting changes analogous to those effected by the operations of nature. If therefore we should venture to adopt the treatment at all, against Mr. Freer's proposal we enter our serious protest. Mr. John Bell has spoken sensibly upon this subject, and we are happy to have it in our power to say so.

"It is essential to your understanding this cure by compression, that you observe the following fact: Aneurism is not what it has been represented a fair round bag containing fluid blood, communicating freely with the artery; no aneurism is truly reducible by pressure, aneurism is hard and firm even of a stony hardness; were it otherwise how could it resist compression for months? The aneurism whether true or false, is filled with knotty coagula, and resists perfectly. The very tumour itself serves as a compress for the artery, the pressing down such tumor compresses the artery betwixt it and the bone, and this is the true reason why *a simple compress* and a *general roller* cures aneurism, without those *cunning screws* and *cushions* which the older surgeons invented so ingeniously for making a *partial* compression. The aneurismal tumour being itself hard and firm, conveys the compression to the main trunk of the artery, which is compressed betwixt it and the bone, while the collaterals lodged among the sound and fleshy parts escape the pressure."

On these accounts it becomes a question whether compression of the sac is not preferable to that of the artery. In the cases upon record it was made at this part, but not by a tourniquet occasionally removed to save the limb from a threatened gangrene, while at the same time all the newly obstructed channels were re-opened to the blood, but by a roller applied *not too tightly*, and in such a way as that the *compression might be equal; then left untouched for at least eighteen or twenty days, and gradually tightened* at each succeeding operation; while venesection of the leg was employed *in preference to the relaxation of the bandage.* Such gross deviations from this ingenious and skilful process, as those implied by Mr. Freer's plan, entitle the latter to no promise of the reputation which Guattani's cases have deservedly attained. Yet these, which resulted from the most artful and gradual advance upon the disease, are quoted forsooth to usher in and to countenance a practice, of such clumsiness as to be hardly reducible to practice, of such violence as to require remission from the pain and inflammation which it occasions, and above all of such dispatch that in four or five days Mr. F. reaches the ultimatum of a process, to see the beginning of which cost Guattani as many weeks. This is another of Mr. Freer's proofs "that the chirurgical art does not stand still," but somewhat less conclusive than the case of M'Donald. If then the proposal of Mr. Freer in no respect resembles the treatment successfully pursued by Guattani, and if as we have seen, it is unsupported by experience and destitute of analogy both in the mode of applying the means, and in the effect which those means produce, it would be mere supererogation to seek any farther disclosure of its poverty and imperfection.

But as our author argues in favour of pressure indiscriminately, we shall endeavour to ascertain with what justness he decides its superiority over the ligature. And first we may observe that it is no impeachment of the latter, far less a vindication of the former to admit, that the operations of Hunter and Abernethy have failed. Ligatures not so tight as to injure the coats or entirely to intercept the blood of a vessel, broad tape ligatures or such as include foreign bodies must always fail. But these so far from making against the operation, evince on the contrary the principle of the ligature to be uniform, and give an inexpressible value to the few cases on record, which have succeeded because such imperfections were avoided. It is true that from the supervention of ulcerative in place of adhesive inflammation, owing to the previous inflamed state of the artery, and from the deposition in other cases of ossific matter, the operation by ligature has occasionally failed. But is it an effect of *pressure* to prevent or retard the ulceration of an inflamed part, or to produce adhesion between parts ossified? In short will compression in any case succeed where the ligature might be expected to fail? we can truly say, we are ignorant of such a case.

If the cures by compression and the ligature are, as Mr. F. hints, equal in point of number, the very gross misapplications of the latter demonstrate its higher character from experience. The frequency of the operation gives Mr. F. reason to believe that "this practice has rather gained ground," and we should wonder if it had not. In all the cases of which we have any account, the patient's sufferings were miserably protracted under compression, and nature and art seem to have engaged in a combat for the mastery, to the last critical and precarious, In proof whereof, let Mr. Freer recur to his own experience: (pages 106 and 108.) Where the operation succeeds, a very few weeks replace the patient on his legs, and so far from leaving *lasting tumours and lameness,* enable him to return to his accustomed labour. But the severity of partial and intermitted pressure exceeds all conception, and if our author had put his vyse upon a human leg, both his horses and readers might have been spared much trouble. We know from experiment that it cannot be endured.

Compression however, as well as the ligature is subject to abuse: it is an abuse of it to employ it in particular parts and under certain circumstances. Wherever the soft parts are of great volume relatively to the bone which they envelop, as in the middle of the thigh, calf of the leg, &c. it is necessarily undefined in its operation, and is less likely to do good than mischief. When on the other hand, the artery lies shallow and contiguous to a bony superficies, as at the wrist and instep, it is direct and executive. When an aneurismal tumour is very bulky, presenting a thin and livid integument, and much œdematous tumefaction of the limb, the constitution is generally in a state of great and permanent irritation, and time enough is not allowed for the slow process of a bandage. This objection does not hold against the legerdemain of Mr. Freer, and *how much less* in such circumstances the constitution might resent the injury, we cannot pretend to say. The case most favourable to one mode, promises best for the other. In the aneurism from bleeding we are opposed by no morbid diathesis. Aneurisms of the wrist, groins, ancle and instep, are those in which we should feel strongly disposed to use pressure in the infancy of the disease. But it is a principle never to be forgotten, that pressure must be general and not insulated, equable and not excessive. Nature's processes of spontaneous reparation are the proper studies and models for our imitation. Hence the degree of pressure as it appears to us, should be just that which sets up the adhesive process in the internal coat, already morbidly susceptible, and not that, which by injury to the surrounding parts induces effusion exterior to the vessel. For the entire and permanent closure of an artery is by the medium of lymph shed into its cavity, and is not the result of a mechanical approximation or contact of its sides.

Some showy plates, the drawings by a pupil of Mr. Freer, ornament the volume; we like best the representations of the fungus hœmatodes. The figure of M'Donald shocks all proportion; the coloured engravings of arteries convey a very inadequate or false idea of natural appearances, and are rather of the sign-post complexion.

We must here close our remarks: what we have said must satisfy our readers that our criticism at setting out was not superfluous. We would advise Mr. Freer to point his attention in future to practical improvement rather than experimental enquiry; and if it fall again to his lot, and probably it may, to meet with a case as creditable to himself and valuable to the profession as that which is the substance of this splendid quarto; let him dispose of the jewel after his own taste—it is not for us to choose, whether it shall be set in gold or pewter.

Art. VIII. *Treatise on Pulmonary Consumption, in which a new View of the Principles of its Treatment is supported by original Observations on every period of the Disease. To which is added an Inquiry, proving that the Medicinal Properties of the Digitalis, or Fox-glove, are diametrically opposite to what they are believed to be.* By James Sanders, M. D. &c. &c. 8vo. pp. 319. London: Longman and Co. 1808.

THE latter is the most novel, and if accurate, the most valuable part of this volume. Phthisis pulmonalis (according to Dr. Sanders's comprehensive definition) consists in inflammation and its consequences, occurring in the substance of the lungs. Inflammation in this, as in other parts of the body, goes through several stages or processes, which as they differ much in their local actions and constitutional affections, require remedies of different properties; a medicine which is useful during the local distention of vessels in the commencement of inflammation would be ill adapted to mortification which follows it. Dr. Sanders has attempted to ascertain precisely the qualities of digitalis, as the best mode of discovering the stage of consumption to which it is applicable, and the principles by which its exhibition ought to be regulated.

Every body knows that fox-glove is considered a direct sedative, or in other words, that it produces an immediate diminution in the frequency of arterial pulsation. It is equally well known that Dr. Beddoes and others have contended, that although digitalis diminishes the frequency, it increases the force of pulsation, and that therefore instead of being classed among the sedatives, it should be placed with stimulants and tonics.

Dr. Sanders pronounces both these opinions to be wrong. He contends that fox-glove is a powerful stimulant, that it increases the strength and frequency of the pulse, and if continued sufficiently long produces flushed face, head-ache, hot skin, restlessness and all the symptoms of febrile action. This opinion Dr. S. has drawn from a multitude of experiments and observations made on the healthy and on the sick. It is not the immature opinion of a few months duration; it has long been his favourite theme of conversation in private, and of debate in public assemblies; he has followed the inquiry with a degree of industry, ardour, and zeal, rare in these times of apathy; he has endeavoured to call out opposition wherever it lurked, and to overcome it. He has hunted after proselytes in the great medical school in which he lives. He has argued with those who are near him till his arguments have produced all the impression that their importance demanded, and he has now challenged those at a distance through the great speaking trumpet of the press.

It is unnecessary to lay before our readers all the documents which are here produced to prove the author's opinion. It will suffice to select such parts as afford the best idea of the evidence on which it rests, and the practical conclusions to which it leads.

"I now resolved to observe what might be the effects of digitalis on the healthy system, if it were taken daily for some length of time.

To know what my pulse was in my usual state of health and manner of living, I kept a table of the number of pulsations counted daily for many weeks before breakfast and before supper. In the morning before I took any exercise my pulse was uniformly 60, feeble; and at night after sitting about two hours it was 56, feeble.

This being ascertained, I began 24th May 1805, to take fifteen drops of the tincture of digitalis in a wine glassful of cold water, night and morning. The first three days my pulse was about 70 in the morning and 66 in the evening, during which time I felt myself uncommonly well, except I thought that I sometimes felt slight shifting pains in the breast.

"My daily reports after the 27th on which I took 25 gtt. are; May 28, I passed a restless night being hot and feverish, and the pain of breast which I had been troubled with was more severe than ever this morning, and from the ensiform cartilage upward, my breast was sore to the touch; pulse before breakfast 76 and strong, and in the evening about 70.—26th I was harassed with tormina the preceding night after going to bed, which did not abate before three o'clock in the morning. I had a sense of weight in the head and of uneasiness in the breast all day; pulse as yesterday.—30th I slept but little during the night, and my mind had somewhat of that restless activity which I have experienced after taking too much wine, but particularly after taking tinct. opii; pulse about 80 in the morning and 90 in the evening, during this day I took 50 gtt. of the tinct. digitalis.—31st I was very drowsy when I went to bed, but did not sleep soundly. I had a troublesome sense of weight in the head, which was relieved in the forenoon by an attack of epistaxis. I continued drowsy and incapable of study till the evening; appetite still impaired, pulse nearly as that of last night. My taking of the digitalis was now given up.—June 1st. my head-ache still continued, I slept better during the night, appetite still impaired and mind incapable of study, pulse nearly as yesterday. I took a laxative composed of aloes and hyoscyamus, and adopted an antiphlogistic regimen.—2nd I slept well during the night, appetite impaired, pill operated gently during the day: on the 3d some degree of head-ache and unsteadiness of mind were still present, my pulse about 78 and pretty full. In eight days from the first of June, the pulse returned to its usual standard."

We next extract one of the cases of disease, in which Dr. Sanders considers digitalis to have been exhibited to an improper extent.

"March 4th 1805. Mr. ———, aged 36, a very tall man, by trade a mason, is greatly emaciated; his eyes are pearly white, his features sharp, face pale, he has high broad shoulders, short neck, and stoops much; his mind is remarkably despondent; he is much distressed by a cough, his sputa are completely puriform; he complains of some pain in the right side, though he lies with ease on either and he has a sense of tightness across the breast, his inferior extremities

extremities are œdematous. There is now beside him his son aged 9 years, dying of phthisis pulmonalis which had already been fatal to his other children. This man has laboured under various forms of this disease for above three years. The means employed have afforded him transient relief, but never the hopes of recovery; his pulse is about 75 and feeble. Mr. Arrindell prescribed for him the digitalis, he began by giving 15 gtt. thrice a day, and wrote down whatever occurred. His cough speedily became more distressing, his breast more pained, his nights more restless. His pulse was immediately stronger, and daily with little variation became more frequent. Sometimes the dose quickened and strengthened the pulse immediately, at others particularly after the medicine had been taken for some days it produced no alteration. At one time it rendered the pulse more irregular, but in general this irregularity subsided and left the pulse more full and frequent.

"In the mean time his appetite was much improved, and vomiting, which formerly annoyed him much, had not attacked him. Febrile symptoms however became more violent, and about the end of a fortnight his pulse varied from 100 to 120 full and strong, for some days he complained of a head-ache, which was at length so severe, that we were under the necessity of employing the antiphlogistic treatment, by which it was removed in a few days, but on renewing the administration of the digitalis, these inflammatory symptoms were reproduced with aggravation, and accordingly we no longer persevered in the prescription."

The following case gives a clear idea of the mode in which Dr. Sanders exhibits digitalis, and the period when he thinks proper to put a stop to it.

"Mr ————, aged 28 years, a man of a spare figure, irritable temperament and acute mind, one of those printers called compositors, was in the end of October 1806, affected with pain in the wrists, seated seemingly in the first row, or as Dr. Barclay would more correctly say in the proximal row of the carpal bones, whence the pain darted upwards as far as the shoulder, and on account of it the hands and fingers could not be moved. The left hand was at first most severely affected, it swelled first and became œdematous. A slight degree of fever and very great irritability were present, the pulse was in general about 80, sharp and irregular; pressure on the diseased parts produced extreme torture, which was terminated by fainting. During a mild antiphlogistic regimen and vesication of the back of his hand and wrists by the meloës vesicatorious, the pain gradually abated, and the limb returned to its healthy state. Next the other wrist and hand, which had been but slightly uneasy for about three weeks from the commencement, began to take the same troublesome course; this limb was treated in the same way, and the pain of the wrist was in three or four days, very much alleviated; an enormous œdema supervened, and it was thought that puncturing would be necessary to prevent the bursting and sphacelation of the integuments; pulse about 80 and feeble. In the mean time to the more formidable external operation, I preferred the internal use of tinct. digitalis, of which conveyed in water and simple syrup, he took 15gtt. night and morning; this raised the pulse to above 90 and strong, and in the space of five days, completely removed the œdema, the integuments became shrivelled like a dry and empty bladder. A cure however was not thus affected, for the pain in the same wrist now returned with as much violence as ever; the use of the digitalis was discontinued, and vesicatories again employed with cathartics and low diet.

"On the evening of the second day after this I was called to him, because he thought himself in the greatest danger. His pulse was uncommonly strong and beating 114 per minute, his looks were anxious, face flushed, respiration interrupted, short and painful. This afternoon he was seized with an inexpressibly acute pain of the left side which preceded the symptoms just mentioned. He was he said somewhat subject to pains in the side, and some years before, a pain which distressed him had been removed by venæsection. Since last visit, he had taken four powders each containing ʒiss. of the compound powder of jalap, which had not moved his bowels in the least. The pain of the wrist still continued though the blister discharged copiously. ℥xxi. of blood which exhibited the buffy coat, were now abstracted from the arm, this gave him instant relief. He was ordered to repeat the powders one every six hours till his bowels were freely opened.

"Next day he continued easier, but still felt the pain of the side and was sick at stomach, he had taken other three powders which had only produced one scanty stool. He was now ordered two powders containing each one scruple of jalap and five grains of the submuriat of mercury; the one to be swallowed six hours after the other if necessary, which two, and the same quantity on the second day after were taken, before his bowels were freely opened. This free purgation at length removed the pain of the side, and the œdema of the affected limb, but not its pain. This pain however daily diminished, while the œdema returned, and in the beginning of December was very great. As the pulse was now reduced to about 70, and every symptom of inflammation absent, he resumed the digitalis in smaller doses than formerly, which in a few days rendered the pulse full and about 90, and removed the œdema. From this time he began to use his right hand again, he rapidly recovered his general health and the use of his hands, and is now (June 4, 1807,) almost as well as ever he was."

The most annoying questions to reviewers, are questions of fact. They cannot appeal to their experiments and their observations, because as they speak with a mask on, no authority can be given for such statements. They must take the fact as they find it, and out of opposing evidences, unsound inferences, and all the errors of the unfortunate author who has fallen into their hands, contrive to obtain an excuse for speaking in their own persons.

The orthodox opinion is that digitalis is a direct sedative. It is needless to argue in behalf of this doctrine. Almost every medical man holds it, and is in possession of the evidence for it; but every man is not aware of what may be said against it. It is an old statement, but it is a very true one, that men form their opinions without having considered the whole argument on both sides. They should count up the argument for and the argument against, just like the debtor and creditor sums of a merchant's ledger.

As we are aware therefore how partially men are in the habit of drawing their inferences, and as we are desirous of making people reconsider their old opinions as often as possible, we have no objection to make our work the vehicle for that mass of fact and reasoning which has been collected in opposition to the commonly received notion about the properties of fox-glove. Those of our readers who are prejudiced against the opinion which we are about to advocate, will perhaps think that some of our reasonings are feeble; but they should remember that these reasonings may appear feeble only because they are new to them. That many of those arguments on other subjects which they now rest upon with confidence, appeared as feeble when they were as new, and that, *cæteris paribus*, old arguments are the most convincing, because by habit they have become associated with feelings of belief.

Those who believe that digitalis is a stimulant, reason, or may reason, thus—In the first place it is universally acknowledged that fox-glove is a powerful stimulant to the lymphatic system of vessels, and it is very improbable that a medicine which possesses the property of considerably increasing the action of one set of vessels, should possess the opposite property of considerably and directly diminishing the action of another set. The whole materia medica does not furnish a similar example, and therefore as far as analogy can go, it goes against such a supposition.

In the second place, those who have seen much of the exhibition of fox-glove, have learned by experience, that there are certain periods and certain cases of disease in which this remedy is more likely to succeed than in others, and these periods and cases are strong evidence of the stimulating power of fox-glove. Thus says Dr. Withering, speaking of the use of this medicine in dropsy,

"It seldom succeeds in men of great natural strength, of tense fibre, of warm skin, of florid complexion, or in those with a tight and cordy pulse. On the contrary, if the pulse be feeble or intermitting, the countenance pale, the lips livid, the skin cold, the swollen belly soft and fluctuating, or the anasarcous limbs readily pitting under the pressure of the finger, we may expect the diuretic effects to follow in a kindly manner."

Now we should hardly expect such to be the fact, if digitalis were a direct sedative. We should on the contrary expect that when the fibre was tense, and the constitution strong, the patient would be best able to bear the debilitating effects of a sedative medicine; and on the contrary, that when the pulse was feeble and intermitting, the face pale, and the skin cold; whatever advantage the patient might derive from drawing off the water would be more than counterbalanced by the debilitating powers of the medicine; that the weakness of arterial contraction effected by the medicine, would produce a gaping of the exhalent extremities, and that as fast as the water was taken up by the absorbents, it would be poured out by the arteries.

To make the argument still stronger, we may enlist in our service Dr. Hamilton, whose book is reviewed in another part of our number; this evidence is the more valuable to the disciples of Dr. Sanders, because it is the evidence of an enemy. Dr. Hamilton writes thus:

"An attentive consideration of the causes which seemed to prevent this operation of foxglove, [that of retarding the pulse] by which I have so often been foiled, has convinced me that they are to be found in the presence of inflammation, or of a tendency to inflammatory action, of the vascular system. In dropsy where this disposition is scarcely ever present, the circulation is commonly retarded by the use of digitalis; and in hæmorrhage, where this remedy produces more decided benefit, perhaps, than in any other species of disease, after the vessels have been emptied by repeated losses of blood, the depression of the pulse is almost invariable:—I presume, also, that in health, no great difficulty would be found in causing this effect of foxglove. But in pneumonic inflammation, where the abatement of the impetus of circulation and the increased activity of the absorbents would speedily cure the disease, until considerable loss of blood has taken place, these favourable consequences of the exhibition of this plant will be looked for in vain. I am aware that authority of no mean weight can be adduced against this decision. I have, however, been too often disappointed in my hopes of sparing the further loss of blood in inflammatory affections, and especially in pneumonia, by the agency of digitalis, not to be satisfied, that however respectable the authors by whom it is supported, the opinion of the powers of this remedy over inflammation is incorrect. Towards the latter period of pleurisy indeed, when bleeding proportioned to the severity of the attack and the strength of the patient, shall have been premised, and when vomica, notwithstanding, seems likely to take place; or, if great secretion of mucus be present, and effusion apprehended, this remedy will then be found to deserve the full extent of Dr. Currie's encomium upon it.." Pp. 100, 101.

"In consumption, if we endeavour to restrain the impetus of the circulation by the foxglove, the same circumstances will be found to obtain in a degree proportioned to the inflammatory disposition of the vessels. When the pulse is hard and bounding, the countenance flushed, the heat and thirst considerable, and the difficulty of breathing—accompanied with pain of the chest—urgent; little advantage will be derived from the use of this remedy, until these symptoms shall have been removed; nor, indeed, can the pulse be readily or at all reduced in frequency when they are present, unless the digitalis be given in such ample doses as to produce unpleasant effects." Pp. 102, 103. Now it is almost unnecessary for us to observe, that if digitalis be a direct sedative, it ought to be peculiarly adapted to an inflammatory state of the system, and that it should be much more efficacious during inflammation, than during a state of debility which had been produced by the disease and its remedies.

In the third place, Dr. Gregory of Edinburgh, has repeatedly observed, that duringthe exhibition of fox-glove the retardation of the pulse was not apparent; but that a few days after the omission of the medicine, the pulse became considerably slower. Some have explained this phænomenon by the supposition that in these cases the digitalis accumulated in the system. If this had been the case, the effects of the medicine would surely have appeared at the time when its use was omitted, and not several days after, when a considerable part of it, and perhaps all had escaped from the system. It is much more probable that in these cases the rapidity of pulsation was supported by the stimulating power of the fox-glove, and that the pulse fell because the stimulus was withdrawn. A fact which is explicable only by a given theory is an argument for that theory.

It is necessary now to let Dr. S. take the argument into his own hands:

"Physicians had not been accustomed to conceive, that any substance whatever could excite the symptoms of inflammatory fever, and that a diminution of the force and frequency of the pulse would ensue as a consequence; therefore, during the use of digitalis, when inflammatory symptoms occurred, they attributed these symptoms to the disease; but the depression of the vital phenomena which supervened, they deemed the peculiar effect of the operation of the remedy.

"They may have been deceived also by observations on the pulsations of persons extremely debilitated: for when, from mere depletion, as from the loss of blood, &c. the body is very much debilitated, and the pulsations are exceedingly rapid and feeble, all stimuli diminish their number, by increasing the force and duration of arterial contraction. What physician has not observed in typhus, that a few glassfuls of wine have diminished the number of pulsations by 20 or 30 per minute? nay, I would ask, what man is there, who watches the phenomena of disease, or is acquainted with the effects of those accidents to which our bodies are daily exposed, but has, in almost numberless instances, witnessed similar effects on the pulsations from the administration of strengthening remedies?

"In similar circumstances the digitalis will, more conspicuously than any other substance, diminish the number of arterial contractions, and invigorate all the functions. I may here however add, that very rarely, in any state of constitution, does the digitalis not instantly, when taken, accelerate the pulse; we shall find, that a few drops in the morning will instantly raise a weak pulse from 110 to 120; and that the same pulse will in the evening be as low as 70; this event occurred to my notice in asthmatical affections which I supposed to arise from œdema of the lungs.

"The phenomena of the pulsations during the administration of digitalis in dropsical affections, may have also contributed to the belief of the sedative virtues of this substance.

"When a watery effusion is collected in any of the cavities or parts of the body, it becomes, in some degree, an irritating cause, but when in the thorax, it most remarkably accelerates the pulsations, after this effusion is removed by any means, the pulsations will diminish in number; this I have often observed, but it is not the effect of the digitalis in diminishing the number of the contractions of the heart and arteries, but it happens because the irritating cause, the water, is removed, and the effect on the pulse is different from that which might have been observed, if the water had been removed by the art of the surgeon. In order to elucidate my meaning, let us suppose, that a person's pulse is, in a given state of body, 80 and feeble, but that water being collected in the thorax, has raised it, and keeps the number about 120; if the water were drawn off by means of the appropriate instruments, a trocar and canula, the pulse, after the immediate effects of the operation had subsided, would be 80 and feeble; but if the water were removed by the digitalis, the pulse would be about 95 and pretty strong, so that the digitalis would *virtute sua* have augmented the force and frequency of the pulse; and this fact it was, I imagine, which made Kinglake declare, 'that a pulse a pulse at 90, soft, full, and firm, is the *sine qua non* of the beneficial effects of the medicine'." Pp. 291—294.

"If digitalis promotes the healing of a sore, or the absorption of pus in the lungs, the consequent diminution in the frequency of the pulse is to be ascribed to the removal of the cause, not to a sedative power. In like manner, in convalescence from all diseases, in which the pulse has been morbidly accelerated, it gradually resumes its healthy number; but is this to be ascribed to a sedative power?" P. 296.

Such is the reasoning by which Dr. Sanders and those who think with him defend their peculiar opinions respecting the properties of foxglove. His disciples are respectable both by their numbers and knowledge. Some have taken and others are daily taking important situations in the practical duties of their profession. They carry with them the peculiar opinions of their leader, and will doubtless diffuse them by the influence of their learning and authority. The question before us is a practical and therefore an important one. It calls loudly for investigation, because the opinions which are now scattered through the world must influence the conduct of many on those important occasions when the question is one of life or death. Experiments should be made, precise observations be multiplied, and the question brought speedily to an issue. Let the infant be crushed in its feebleness, or if lawfully begotten, be acknowledged by the world.

ART. IX *Observations on Morbid Poisons, Chronic and Acute*, By Joseph Adams, M. D. F. L. S. 4to. pp. 406. 1807.—Callow, London.

TO the physiological opinions of Mr. Hunter, so ostentatiously displayed, and so parasitically applauded by Dr. Adams, we have already offered various objections: to the pathological doctrines of the same distinguished author, received and recounted with similar obsequiousness of feeling, we are compelled also to direct our opposition. If it be true, that the interests of medical science are promoted by the diffusion of correct ideas respecting the healthy functions of the human system, it will not be doubted that accurate information in regard to the nature of diseased actions, as more directly influencing our procedure in practice, is of the highest importance. The course which Mr. Hunter has chosen in pathology is circumscribed, but in his progress he has attempted to establish principles applicable to all diseases, and has connected with his own more immediate inquiry, speculations, which may assist or impede us in the treatment of every morbid condition. In the disease to which he has confined himself, the cases are, however, more numerous (such is the incontinence of the age in which we live) than those resulting from any other morbid influence; and as it is fraught with consequences the most loathsome and dangerous when neglected or improperly treated, it must demand in theory as well as practice, the greatest attention.

As the diseases described and explained by Dr. Adams are stated to arise from morbid poisons, it may be proper to mention the restricted sense in which this term borrowed from Mr. Hunter, is employed. Many may have been accustomed to consider every cause of disease a morbid cause, and every power so injurious to the body of man as to deserve the name of a poison, to be a morbid poison; hence arsenic, nightshade and the destructive fluids of the viper and cobras de capellas, might be conceived to merit the appellation, as much as the virus of the small pox or venereal disease. It might also have been supposed to signify all poisons, the result of disease; but those only are admitted to be morbid poisons, which " have a power of exciting a similar disease by the secretion of matter which will have the same properties " Animal poisons are therefore divided into natural and diseased, or according to the *amended* phraseology of Dr. A. into original and diseased; and the latter are subdivided into morbid and those not morbid, according to the restriction just stated.

Of all the morbid poisons the venereal is unquestionably the most perplexing and severe: it is this poison, of comparatively recent origin, the libidinous indulgencies of the earlier periods of the world having been embittered by no such scourge, which, under all its varied aspects, is discussed by Mr. Hunter in his justly celebrated treatise; and which also chiefly engages the attention of his expounding panegyrist in the work before us. The first attempt to ascertain the laws by which this baneful poison is regulated in its action on the system, was made by Mr. Hunter: the experience of three hundred years had furnished an ample store of materials, and his own extensive opportunities of tracing the disease, improved by a high degree of penetration, most essentially contributed to the advancement of his attempt; but the subject then was, and still continues we fear, too uncertain and obscure for successful generalization: we are continually hesitating respecting the name and nature of many common appearances, and the most eminent practitioners in the metropolis often give opposite decisions in cases submitted to their judgment. Even Dr. Adams himself, who has been thoroughly infected by all Mr. Hunter's excellences, to the production of a full *eruptive action*, has not been spared those embarrassments which his less favoured brethren have encountered*: yet afterwards, in speaking of Mr. H.'s discoveries he avers, that, "to use the author's expression in the last conversation we had together, the inattention to these leading facts has been the cause of all the uncertainties of so common a disease†."

The great discoveries of Mr. H. consist in the detection of those states of the constitution, which occur during the progress of the disease, or, as Dr. A. often says, "its series and order;" in the view which he has presented of the best time for the employment of the acknowledged specific; and of the mode by which the removal of the complaint is effected. These discoveries, under the title of laws of the disease, are with considerable prolixity and minuteness of illustration detailed in the second chapter of the work on Morbid Poisons: from this chapter the following sketch is condensed. Whether the morbid poisons may be the simple or compound of Hunter, or the infectious and contagious of Adams, three states of the body are necessary to their existence, viz. susceptibility, disposition, and action. The susceptibility may exist in the constitution, or in a part only; the disposition which follows, may be general or partial likewise; and the action which, as an inseparable consequence, succeeds to the disposition, will in extent be proportionate to it. The susceptibility may be prevented or destroyed by rendering the constitution familiar with the morbid poison, or by the action of another morbid poison, or any other cause; the disposition may be suspended by the action of another morbid poison, or by a medicine producing a constitutional action, but cannot by any means whatsoever be destroyed; the action, its requisite precursor being indestructible, when every local and constitutional impediment is removed, must after a certain period, varying according to the nature of the poison, inevitably follow.

Of the three circumstances incident to a part which from the application of morbid poison has advanced to disease, that which occupies the intermediate station is alone peculiar to Mr. Hunter, and in its introduction, if we mistake not, consist alike the novelty and the merit of his opinions. It had been well if Mr. H. had perceived the necessity of defining and absolutely fixing the syphilitic application of this accommodating term: he was aware that in general usage it has a very different meaning, and this we conceive ought to have obtained for it a new definition, if not prohibited its new appropriation. Dr. A. does inform us that Mr. Hunter was one who never "reasoned by definition*," but this will not excuse him for not defining before he reasons. This deficiency, unhappily for our progress, is not supplied by our author: he was ignorant we conclude, that the language is figurative and indefinite; otherwise a definition or a condemnation must have escaped from one, who declares that "it cannot be still necessary to remark that many of our physiological errors arise from the *looseness* or figurative style of our language†:" now according to our best recollections, we have never met with more serious and inveterate cases of *verbal diarrhea,* than those of Mr. Hunter and Dr. Adams, nor any instances in which proper *logical astringents* might have been administered with more happy effects. If any information can any where be found on this dark subject, we think it may be extracted from the following remarks, illustrative of the second aphorism.

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* Morbid Poisons, 2nd edition, p. 38. † Ibid. p. 163.

"If the part or constitution, or both, are susceptible, the disease does not immediately follow the application of the poison either by effluvia or in the form of matter. It requires a certain time, during which the constitution *seems disposing* itself for the future action; for if any other disease interferes, the action of the disease from the infection is protracted; the new disease goes through its usual progress, after which the constitution recommences the process of disposing itself for the first disease, and the action follows after the usual period required for that purpose. As this state of the constitution, from the period of receiving the infection to the commencement of the disease, is often the subject of very important inquiries;—as it is a state in which we can neither cure or prevent the disease which is to follow, but of which in some morbid poisons we can protract the subsequent action, and in others we find it occasionally protracted by incidents which we could not prevent, but ought always to be prepared for,—on all these accounts Mr. Hunter denominated the state of the constitution *the disposition for the future disease.* I hope this explanation will be intelligible if it is not satisfactory. Though I have thought it necessary to offer it because Mr. Hunter's writings cannot be understood without it, yet, as several candid and ingenious men have objected to the term, I shall avoid using it as much as possible."

Although we must confess our ignorance of what the constitution is about when *disposing itself* for future action, we are not quite so much at a loss to comprehend what a person is doing, who is so disposing himself as to establish a community of fame with an author of high posthumous celebrity; nor what is the nature of *a disposition* manifested by one who publishes

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* Morbid Poisons, 1st edit. p. 40. † Ibid. 2d edit. p. 150.

professedly in favour of the cow pox, in order exclusively to *make and sell* an elegant and polished species of variola, under the tickling title of *pearl small pox*[*].

Our objections are not however confined to the term alone; the state which it represents is hypothetical, and it is assumed we contend without any foundation, except the anxiety of Mr. Hunter to resist all interference of a venereal virus, beyond the earliest periods after contamination. The virus is no sooner absorbed than it must be expelled, and disposition only remains to attest by subsequent action its noxious influence. To us no good reason has appeared for thus hastily expelling the morbid cause. That the solids and fluids are subjected to a continual change we do not deny, but the celerity of that change we believe is not so great as was once imagined. The experiments of Du Hamel and Morand in which colour was communicated to the bones of animals by mixing madder with their food, have led to this misapprehension; and the exaggerated conclusions of Mr. John Bell[†], who from his unceasing propensity for extremes has pushed the absurdity to its most glaring degree, have been satisfactorily refuted by the criticisms of Mr. Gibson[‡], and the experiments of Dr. Mc Donald[§]. But even admitting all this to be true of madder, must it necessarily be true of venereal virus? Is that substance which will without pain beautify the bones of a pig, an exact representative in all its habitudes, of the poison which will produce pain and deformity in the bones of a man? who we would ask has detected venereal matter in the *excreta?* and unless it were possible to shew that secretion is merely filtration, who shall pretend to prove, that through every change in the circulating fluids, the virus may not be detained in the system? The length of time intervening betwixt absorption and disease in which no change is perceived, can furnish no valid argument in behalf of the doctrine; for if venereal matter in that state of concentration which belongs to the primary ulcer require seven, fourteen[||] or even sixty days[**] to affect the part on which it is immediately applied, it cannot surely be matter of surprise that virus introduced by the absorbents and mixed with the blood, should occupy six weeks for the completion of its effects. But let us grant to the *antivirus* men that the absorbed matter is expelled with all the rapidity they desire, and let us have the disposing process suspended by the action of a morbid poison, what power will remain to insure its recommencement? we must not look for its renewal from casualties of the constitution, because the action follows, they tell us when the obstacle is removed, in the same number of days precisely which would have been required if no interruption had occurred: it was the virus which in the first instance set the system to work, but the virus being removed nothin gremains to continue the process; consequently we have an effect without a cause; an action without an agent. This assertion must submit to a slight correction for it may be owing to *retro-active sympathy:* granted; for in these days what cannot sympathy in one shape or other be made to perform.

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[*] Vide a popular View of Vaccine Inoculation, showing the analogy of small pox and cow pox, and the *advantages* of the latter!! p. 25.

[†] Anatomy of the human body, vol. i. p. 13.

[‡] Manchester Memoirs, vol. vi. part 2nd. p. 158.

[§] Dissertatio inauguralis, de Necrosi ac Callo. Edin. 1799.

[||] Morbid Poisons, p. 112.

[**] Hunter on the Venereal, p. 218.

We call upon the defenders of Mr. Hunter explicitly to declare, whether disposition consists in action or not: the dilemma in which we think they will find themselves placed, we candidly set before them. If disposition be admitted to be action, as it may be superseded by a morbid poison the effect of which passes necessarily through disposition, two actions must have taken place in the same part at the same time; the possibility of which we shall presently see is denied: if it be not action, then it must be a mere aptitude or condition, requiring a new impulse before any action can take place, and the assistance of virus being rejected that impulse they are bound satisfactorily to exhibit.

Another proof of the exclusion of the venereal poison, and the formation of disposition is derived from the assertion resting as it said on fact, that " when the diseased action has taken place in one order, the skin for instance, and has been cured it *never* appears again in that order of parts from the same stock of infection *." Against this assertion we might adduce many well attested instances. The experience of Mr. Bell has taught him that the disease will break out again, when, as to appearance it had been removed †; and the cases detailed by Dr. Clutterbuck in his very sensible and argumentative " remarks," are conclusive against the position ‡. With this evidence our own observation entirely coincides as well as that of many surgeons, extensively consulted on account of the disease, to whom the position has been referred. Lord Shaftsbury has shrewdly remarked "that the most ingenious way of becoming foolish is by a system;" it is also the most hopeless, for the *blindness* induced by system cuts off the only reasonable prospect of amendment.

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* Morbid poisons, p. 158. † On the Venereal Disease, vol. ii. p 159.
‡ Remarks on Hunter's opinions, p. 20.

But the most decisive refutation of the facts, laws, and reasoning of Mr. Hunter and Dr. Adams, is comprised in the following observations, the accuracy of which, will not we presume be disputed by the latter.

"When in the first instance the chancre is cured by remedies applied to the constitution, the disease does not shew itself on the genitals a second time from the same infection, but on the skin or fauces. If in this stage no mercury is exhibited, the blotches will increase in number and ulcerate, but *if the first are cured by mercury, no fresh ones will appear*. If the bones have taken on the disposition, then in a given time after the mercurial irritation which cured the skin has ceased, the disease will appear in one or more of them. If no attention is paid to these they will ulcerate and other bones, if contaminated will shew the disease likewise: but *if mercury is exhibited so as to cure the first, no others will ever shew the disease* from that stock of infection *."

If any blotches should appear on the skin, or if any affection of the bones should occur after the secondary symptoms of either order have been first observed, which the early employment of mercury could have prevented, some virus must at that time have existed in the constitution; for according to Mr. Hunter's doctrine mercury can only protect by destroying the virus before it has induced disposition. If the virus were not present then disposition must have been at hand, but "as the venereal disposition when once formed in a part must necessarily go on to form the venereal action †," it is clear that an equal number of blotches and an equal extent of bony injury must be produced, whether the specific is delayed or not. In confessing what we need not doubt to be a truth, the preventitive influence of mercury introduced in the stage of the disease above mentioned, Dr. A. has fully admitted, either the detention of virus or the cure of disposition. If the Dr. has taken the liberty *to oppose* Mr. Hunter for making as he thinks a few improper concessions to former opinions, with what reproaches may the indignant spirit of Hunter load his treacherous ally, who obedient to fact and regardless of hypothesis, has conceded to truth the entire system.

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* Observations on Morbid Poisons, p. 158.

† Hunter on the Venereal Disease, p. 314.

The rejection of direct assistance from venereal virus in producing secondary symptoms, has lately been extended still farther; and the office has been consigned to a power, peculiarly Mr. Hunter's, the power of sympathy, or something vastly analogous to it. An author who has signalized himself by exceedingly *warm* endeavours to *starve* the gout from its station in the great toe, and who labours under a *drimutephobia* of the most excruciating kind, thus gravely and luminously writes.

"It is one of the most provident conditions of animal life, that *morbid matter is not a transferable evil.* The motive powers of organic structure effectually bar its diffusion in a material form. They are susceptible of being impressed by it, and morbidly agitated by its influence, but there the mischief stops; and previously to its farther propagation or systematic dissemination it must assume the motive form which vital action imparts to it. In this state it acquires an identity with vital power, vitiating its exciteability, and becoming subject to the same laws of motive diffusion *."

In the terms motive form and motive diffusion, we think we recognize that which we have been accustomed to denominate sympathy, and we suggest with all becoming diffidence, that it may be arranged under the *contiguous* or *continuous*, the second and third species of the second order, the *partial* of the extensive genus, sympathy: that it may either be contiguous or continuous sympathy, we do imagine, but we certainly give a decided preference to the latter, because it approaches the most nearly to that kind of extension, in which the uninitiated can discover no sympathy at all. "The *continuous sympathy* is, where there is no interruption of parts, and the sympathy runs along from the irritating point as from a centre, which is the most common of all the sympathies; and *we have an example of it in the spreading of inflammation* †." Mr. Hunter somewhere speaks of a chain of sympathies; we think he has here stretched it to its utmost extent, and it must be almost impossible to add another link. Perhaps for the last extension we may append the idea of an universal sympathy arising from a partial sympathy ‡

When mercury is employed for the cure of the venereal disease, as it is finally decreed that no virus shall exist, and that action is alone to be destroyed, we have no occasion to estimate the affinities or the chemical powers of this agent. It does appear from experiments related by Dr. Cullen and credited by him, that when Plenk's solution of mercury was triturated with matter taken from a chancre, and the compound applied to a sound person, no syphilitic symptom followed*: the experiment we confess is not entirely free from objections; but since it is equally good with many sanctified in our author's estimation by the authority of Hunter, we hope it will be permitted us to believe, that should the mercury meet with a particle of virus, by an accident the most rare, wandering through the system to the great annoyance of its susceptibilities, the contest which must ensue, would give victory to the mineral, and composure to the constitution. But where the poison no longer exists and yet the symptoms do apppear, we must conclude that it acts "upon the principle of destroying the diseased action of the living parts, counteracting the venereal action by producing another of a different kind †." In attempting the cure "the object then is to excite an irritation greater than the venereal. That this is the mode in which mercury cures the disease is now pretty generally admitted, and is proved by every analogy which we have traced in the manner in which morbid poisons yield to each other, and to various other irritations ‡." The object is farther stated to be to produce such an irritation as will supersede the disease §." The change thus occurring presents a particular instance of a general law first devised and employed for pathological explanation by Mr. Hunter; a law which declares that "two actions cannot be carried on at the same time in the same part or in the same constitution."

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* Kinglake's Dissertation on Gout, p. 23.

† Hunter on the Venereal Disease; Introd. p. 2. ‡ Ibid. p. 1.

This law we strongly suspect has had a mechanical origin, and had it acknowledged any other parent, we fear that it might have been exposed to the severe castigation which, as we have seen, was brought upon Baron Haller, by the mechanism appearing in his explanations. With respect to ordinary matter it is not admissible, for the same mass may at the same time move in obedience to two forces, and therefore have two actions: this is sufficiently evinced in the motion of a projectile; and the particles of which it is composed will be found capable of a third action, if in its flight the body should revolve, as well as describe a parabolic path. It would not be difficult to shew that in the natural actions of the human system this law is contradicted; and if we are not deceived, although in the morbid state some facts may seem to countenance it, yet will it be found opposed by a number much greater. Dr. A. in justification of the law, or as facts from whence it may be deduced, has mentioned several instances of measles and small pox, and measles and scarlet fever being epidemic at the same time; and because no instances are mentioned by Sydenham or De Haën, and only two by Dr. Russel, where the diseases appeared together in the same individual, he deems the silence proof. But to render these cases availing it ought to be shewn, that in each individual, the diseases followed in such quick succession, as to make it certain that if one had not been present, the other would have been there: the same remark will apply to the negative evidence of Dr. Willan in respect to small pox, measles, hooping cough, and scarlet fever, epidemic about the same time. Dr. Willan has asserted that hooping cough and small pox have occurred in the same person at the same time, and that in one instance, hooping cough commenced during the small pox eruption. Dr. A. very flatteringly hints that the disease had lost its specific character, which Dr. Willan might have been supposed qualified to discover; and in the second, paying a high compliment to the Doctor's diagnostic talents, pronounces the cough to be a common catarrh. But for these contradictory incidents, ample atonement is made by Dr. Willan, in the *elegant* experiment conducted at the *small pox hospital;* which elegant experiment consisted in inoculating the same person with the lymph of measles and cow pox, and finding that the former politely kept in the back ground, until the latter had done its duty in protecting the constitution. We know that in several instances small pox and measles have existed together, and that this is the case with small pox and cow pox, every one must allow: of the last fact however, Dr. A. can make a very good use; it defends the law, their identity being assumed as true, or the law being assumed, it defends the identity. The two affections, for one only can be termed a disease, have some similarity we admit, but their difference as to eruption and contagious quality is much more striking than their resemblance. Our idea of their similarity we cannot better illustrate than by borrowing an elegant *pistorian* example from the first edition of the Morbid Poisons, inserted also for the benefit of those possessing the second, in which, strange to tell! it is no where to be found. "Now if this is to be called" resemblance "it will not be difficult to prove that a man and an image of rye dough are the same thing. For though it may be right to take notice of *one striking difference,* namely, that a man can walk and an image must be carried, yet as there are *other resemblances,* we may prefer calling them by the *same name**." The fact which the law is brought to explain, and which led to this discussion, overturns the law itself: the removal of venereal action by mercurial action is gradual and progressive; the mercurial must therefore exist along with the venereal until the latter is entirely overcome; and if these actions were not at certain periods co-existent, the cure must be accomplished in a moment, or it could not be accomplished at all.

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* Treatise on the Materia Medica, vol. ii. p. 448.

† Hunter on the Venereal Disease, p. 344. ‡ Morbid poisons, p. 117.

§ Ibid, p. 118.

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* Morbid Poisons 1st edit. p. 16.

After having sufficiently explained and defended Mr. Hunter's opinions, our author proceeds to impart the information which he has collected respecting affections produced by several other morbid poisons. Of these some are similar to the venereal disease, and have been adverted to by Mr. Bell, as furnishing incontrovertible arguments against the identity of the virus of gonorrhea and chancre: as this position was strenuously maintained by Mr. Hunter, it necessarily obtains the protection of Dr. A. who being dissatisfied with the accounts given of sivvens, one of these complaints, attempted by a personal investigation, to supply the defects. The particulars of his tour into those parts of Scotland most infested by the disease, are detailed with considerable minuteness; and wonderful to relate, the medical gentlemen of Dumfries and New Galloway so "far from feeling jealous at his undertaking to inquire after a disease well understood by themselves, were ready to give him every information, and to listen patiently to all the prolixity of his inquiries and suggestions!!" there are some amongst our *northern* medical brethren however, who do not so willingly admit others to share with them the honours of discovery. The sivvens or sibbens, (deriving its name from a strong resemblance in its small excrescences or fleshy sproutings, to the wild raspberry,) was first described by Dr. Gilchrist in the year 1771. It appears that as in the venereal disease, symptoms which may be termed primary and secondary occur: the first are confined chiefly to the uvula, tonsils, and adjacent parts, consisting of ulcers, which remain when white crusts or sloughs, the result of previous inflammation have been removed: in some cases the uvula and tonsils have been entirely destroyed. The appearances which have been noticed as belonging to the second class are not so regular and uniform as the preceding: the following case related by Dr. A. will convey a tolerably accurate account of them, as well as of the progress of the disease. It is the case of a poor girl who had been ill about twelve months.

"Her first symptoms were mere sore throat and treated as such with little or no advantage. At length the secondary symptoms appearing with some fever were at first suspected to be small pox, till the slow progress of the pustules evinced the contrary, and a surgeon being consulted, at once discovered the disease. From her situation she had not been able to persevere so regularly in her remedies as to produce a complete cure. Her throat exhibited only a dry scaly appearance, in the left arch; whether it had ulcerated I cannot determine, only that the parts appeared to me as if diminished. She had several very hard, dark, coloured scabs, in different parts of her body, cicatrices in other parts, and in others, pustular appearances. On visiting her afterwards some of the cicatrices were breaking out afresh. She has all the symptoms of low fever, and an indolent superficial sinus appears on her neck. Whether these last symptoms were the effect of the disease or of poverty, cannot easily be ascertained. On inquiry, she recollected complaints she had about the pudenda and anus, which had healed under the mercury she had taken, and which she assured us remained well*."

From that part of the sequel of this case which was transmitted to Dr. A. we learn, that having been inoculated with vaccine matter the progress of this severe disease was arrested for six weeks. We regret the termination of the narrative, and we do certainly "*sympathize* with *the author of morbid poisons*, that no more minute history of this interesting case could be procured†." The bones seem to be no otherwise affected by this disease than in consequence of its extension from the soft parts in contact with them, and it is much more quickly cured by mercury than the venereal. From what Doctor A. has advanced on this subject we coincide with him in opinion, "that sivvens is different from the venereal disease, though approaching nearer to it than any poison with which we are acquainted." At the same time from this view of his discriminating powers, we are induced to believe, that if he had felt the same interest, speculative or substantial, in distinguishing small pox and cow pox, that he did in confounding them together, he would have been no less successful than in the present instance.

Respecting the Canadian disease vaguely mentioned by Dr. Swediaur and Mr. Bell, the active inquiries of Dr. Adams have brought to light some important documents. It may not be true that thousands, about twenty years ago were infected by this disease in Canada, but it appears to be sufficiently ascertained that at the Bay of St. Paul, a disease very similar to sivvens appeared; agreeing with it especially in this circumstance that both have been frequently communicated by smoking with a pipe which had been used by an infected person.

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* Morbid Poisons, p. 185.

† On this work we perceive rests the author's hopes of literary immortality: the name of Adams we doubt not, will ere long be swallowed up in that of *Morbid Poisons*, like the names of Homer and Milton in the Iliad and Paradise Lost.

In the following chapter we are presented with a case of yaws very well and very minutely detailed. It is a disease little if at all known in this country, and in Scotland where it is stated sometimes to occur, Dr. Cullen says that he never saw one example of it. In Africa and the West Indies it frequently appears. The subject of this narrative was a young Danish nobleman left in Maderia during Dr. A.'s residence there. The complaint appeared to commence with a slight but regular intermittent, attended during the paroxysm with a spasmodic contraction of the fauces; this was imperfectly relieved by bark and other tonics, and yielded completely to a grain of calomel administered every day for the space of ten days. When the influence of the mercury ceased the fever returned, and during the remission was attended with a pimply eruption on the face and an inflammation of the throat:

"In the space of two days the eruption was universal excepting on his hands and feet, the throat became ulcered, and the glans penis was covered with several small but not painful ulcers. For several days the ulcers spread on both these places and the pustules continued to increase as his fever returned, and the throat and pustules of the face were attended with considerable pain. The latter had arrived in less than a week to the size of small-pox pustules; and the interstitial part of the skin being redder than usual, gave the whole very much the appearance of that disease. The figure of the pustule was indeed somewhat flatter, or rather more horizontal, the edges rising perpendicular from the skin and the surface being plain. There wanted also that appearance of indentation which very soon discovers itself in small-pox and frequently in chicken-pox also. The throat had precisely the appearance of a venereal sore throat, but was more painful*."

'In the course of a month some of the pustules, of an oval form, had increased so as to be two or three inches in the longest diameter, and the pain had become so excessive as to render life intolerable. In six months from the commencement of fever and four from the first appearance of eruptions, the disease seemed to yield to the energies of the constitution; for excepting aqua phagedenica applied with advantage to the ulcers in the throat, corrosive sublimate which the stomach could not bear, and bark with opium exhibited occasionally by clyster, nothing appears to have been tried.

We shall not be required to offer many remarks on the succeeding chapter. The author is there engaged with Mr. Abernethy in rendering the diseases resembling syphilis, still more

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* Morbid Poisons, chap. xvi. p. 196.

obscure than they appear to be from the essay on that subject; and that obscurity, in which we fear they both too much delight, we shall leave them uninterruptedly to enjoy. Dr. A. is also engaged in artfully but unsuccessfully defending himself against the well directed and decisive criticisms of Dr Clutterbuck. The attempt to cure the venereal disease by acids, mentioned in this section, does not find an advocate in Dr. A. this might have been expected as he had previously decided that no remedy has any power over it except mercury: but we think he might with advantage to his character for candour, should it be already established, have withheld an insinuation that the late Dr. Currie mistook scrofula for lues venerea, and was in other respects ignorant of the disease; and we do not perceive the necessity, much less the compliment, of the call upon Mr. Pearson to mention every circumstance of every case which he may pronounce to be venereal, "that *the reader may judge* of their nature." If *the physician* to that most humane and patriotic institution, which hourly disseminates "the most formidable contagion with which we are acquainted," should not be permitted to speak of small-pox, to one who is not a physician to a small-pox hospital, without exhibiting each particular of the case to shew that he knows the disease, we strongly suspect that some unutterable expressions of offended superiority would make their appearance: and why should not a similar privilege be granted, we ask, to a surgeon of the Lock Hospital. When we contrast the physiological and pathological talents of Mr. Hunter with those of Dr. Currie and Mr. Pearson as displayed in their writings we are not disposed to think that the question respecting the antisyphilitic power of acids, would at this time have been in a less perplexed and hypothetical state had it obtained this all-achieving aid. Whilst we admit that he might have been an useful associate we cannot join in the following *lugubrium* of Dr. Adams; "but J. Hunter was no more! and this calm, this patient inquiry, this silent investigation, without engaging in party violence which warps the understanding and almost teaches us to deceive ourselves, was too much to expect a second time in a century."

Our limits will not admit of an analysis of the dissertations on leprosy and the itch insect sufficiently extensive to render it valuable to our readers. We shall omit noticing the particulars of the second part of the Observations on Morbid Poisons including diseases which arise from their influence and are attended with critical fever, as the subject of fever will soon receive very minute attention. We shall pass over the sentiments of Dr. A. respecting cow-pox, briefly noticed in this division and previously termed the *invaluable* Jennerian discovery* (a sly *equivoque*, we suppose, for he does not now think it even worth recommending) as these sentiments will ere long undergo a separate critical investigation.

In concluding this article we may remind Dr. A. that in common with several of the followers of Mr. Hunter, he has by the extravagance of his admiration challenged us to a criticism more severe than might otherwise have been attempted. Had Mr. Hunter been represented as an assiduous, ardent, and successful anatomist, and as an ingenious, intelligent, and scientific surgeon, whose opportunities in these branches were great, and actively improved, his claims would have been cheerfully admitted. But when we are called upon to acknowledge him as the author of all physiological and pathological science, and in their pursuit as alone treading in the paths of Newton and of Bacon; when we are required to believe, in opposition to the strongest evidence, that "he had no turn for fanciful speculation and had ever an aversion to all hypothetical reasoning†" we are compelled to examine and contest the unjustified demands. Why such panegyrists should exist, and why such panegyrics should be written, we shall not pretend to divine. We are informed that "authors do not *always* write with a view to the improvement of knowledge ‡:" with what *other views* they *may write*, none we think can more fully disclose than the author of "Observations on Morbid Poisons," and the author of a "Treatise on Strictures in the Urethra and in the Esophagus." Should Dr. Adams wish again to appear before the public with the view acknowledged, or with any other view, in selecting his subject we request him to bear in mind the following hint of Lord Bacon, in his apprehension second *only* to Hunter. "Postquam homines dedititii facti sunt, & in unius sententiam (tanquam pedarii Senatores) coierunt, Scientiis ipsis amplitudinem non addunt, sed in certis authoribus ornandis & stipandis *servili officio* funguntur."

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* Morbid poisons, p. 21.

† Home on Strictures in the Urethra, &c. Introd. p. 7.

‡ Morbid poisons; pref. rem. p. xi.

LIST OF MEDICAL PUBLICATIONS.

The Edinburgh Medical and Surgical Journal, exhibiting a concise View of the latest and most important Discoveries in Medicine, Surgery, and Pharmacy. No. 13. 3s.

An Essay on the Venereal Disease, and its Treatment. By W. Blair, A. M. 3d edition. 8vo. 10s. 6d. boards.

The Principles of Surgery. By John Bell. Vol. 3. part 1. 4to. 2l. 2s. boards.

A View of the Nervous Temperament. By Thomas Trotter, M. D. &c. &c. 2d edition. 8vo, 7s. 6d. boards.

Observations on the Inflammatory Affections of the Mucous Membrane of the Bronchiæ.—Comprehending an Account of the acute Inflammation of the Parts—of Peripneumonia Notha and of Chronic Coughs, &c, By Charles Badham, M. D. Lecturer on the Practice of Physic, &c. and Physician to the Westminster General Dispensatory. 12mo 3s. 6d boards.

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Medical Observer, or London Monthly Compendium of Medical Transactions. Nos. 5 to 7. 2s. 6d. each.

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Essays on the Diseases of Children, with Cases and Dissections; Essay 3 on Hydrocephalus Acutus, or Dropsy in the Brain. By John Cheyne, M. D. Imperial 8vo. 12s. boards.

Letter to the Right Honourable Sir Joseph Banks, Bart. F. R. S. on the Causes of the prevailing Discontent, Imperfections, and Abuses in Medicine, from Thomas Beddoes, M. D. 8vo. 4s. sewed.

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Hints for the Consideration of Parliament, in a Letter to Dr. Jenner, on the supposed Failures of Vaccination at Ringwood; including a Report of the Royal Jennerian Society on that Subject, after a careful public Investigation upon the Spot; also containing Remarks on the prevalent Abuse of Variolous Inoculation, and on the dreadful Exposure of Out-Patients attending at the Small-pox Hospital. By W. Blair, Surgeon of the Lock Hospital, &c. 2s. 6d. boards.

Observations on White Swellings in the Joints, with Remarks on Caries, Necrossis, &c. in which a new and successful Method of treating these Diseases is pointed out. By Bryan Crowther. 8vo. 10s. 6d. boards.

J. Callow's Catalogue of Modern Medical Books for 1808.

General View of the Atmosphere and its Connection with the Science of Medicine and Agriculture. By Henry Robertson, M. D. 2 vols. 8vo. 1l. 1s. boards.

MEDICAL INTELLIGENCE,

Consisting of brief Communications of Varieties in Disease, and Novelties in Practice is requested to be addressed to the Editors, at Messrs. Longman and Co. Paternoster Row.

Mr. Allan Burns has lately met with a pudendal or perineal hernia of the Bladder, in the body of an aged female. The particulars we hope will shortly be made public.

Several operations for the stone have within the last twelve months been performed by two eminent surgeons in this city, and one of Manchester, with the knife alone. The facility and success which have attended these cases, promise a material simplification of the instruments at present used, and will ultimately set aside the use of the gorget.

The present Number contains one sheet less than the former, (i. e. six sheets) it being the intention of the proprietors to fix the size of each at six sheets and a half.

Reviews of the recent publications of Dr's Clutterbuck, Beddoes, Adams, and Barclay, will appear in the next number.

CORRIGENDA.

P. 29, l. 40.—*For* " crural and inguinal" *read* " inguinal and crural."

P. 107, last line.—*For* " muscular" *read* " vascular."

P. 110, l. 36.—*For* " contracted" *read* " concentrated."

THE

LONDON MEDICAL REVIEW.

AUGUST, 1808.

N° III.

ART. I. *A Letter to the Right Honourable Sir Joseph Banks, Bart. P. R. S on the Causes and Removal of the prevailing Discontents, Imperfections, and Abuses in Medicine,* From Thomas Beddoes, M. D. 8vo. pp. 148. 4s. Phillips, 1808.

THE inevitable law of necessity, says Dr. Beddoes, places men for a greater portion of their days at the mercy of physicians than of publicans and postilions; yet our peers and country gentlemen do not seem to suspect that their accommodation on the journey of life, depends so much upon the state of medicine as upon the inns and roads in which they take so deep an interest. Of the importance of a good education to the guardians of the public health no one can be insensible, and of the abuses, which exist in this as well as other countries in the practice of medicine, no intelligent man can be ignorant. But it is one thing to point out abuses, and another to remedy them; and of all the plans yet suggested on the subject of medical reform, we have no hesitation in saying there is not one which could be safely adopted. They all proceed upon the principle of lessening the number of practitioners of medicine, rather than that of affording increased facilities for the study of the science itself.

The present letter, addressed to Sir Joseph Banks, contains a statement of the abuses which exist in the medical world; and the defects of the common mode of education, are strongly pointed out. Dr. Beddoes never writes a dull book. A vigorous imagination, considerable knowledge of men and books, and a happy facility in the use of language, render him an author whose works will always be read with much pleasure and some profit. It is provoking that a man who writes so well should argue so inconclusively.

Though Dr. Beddoes does not seem to entertain a very high opinion of the College of Physicians, or of their plan of medical reform, yet he seems to agree with them in the great principle of prolonging the period of study assigned to the candidates for diplomas—a principle of the truth of which, abstractedly considered, no one can doubt.

Dr. Beddoes proposes that the medical student should employ six years at different universities, or schools of physic, in the following manner:

"First year. *Dissection, anatomical lectures, reading, drawing, and comparison of anatomical engravings with the objects in nature.* For relaxation, as much as for instruction, *a course of chemistry and elementary reading*:—this for winter. In spring and summer, *a course of comparative anatomy, dissection of animals, botany and physiological reading*, till the winter of the

"Second year. *Anatomy exactly as before, attendance on clinical lectures in surgery*; if none are given, *close study of surgical cases particularly of surgical accidents at first; morbid anatomy practically*, by every opportunity from this time forward. In spring, summer, and autumn, *practical chemistry, pharmacy, botany, materia medica.*

"Third year. In winter—*Anatomy and surgery* still; but external diseases now more than accidents. Spring, summer, and autumn—*Midwifery, medical jurisprudence, comparative anatomy, physiology*, and the other before-mentioned pursuits occasionally.

"Fourth year. *Anatomy to be kept up, lectures on the practice of medicine, clinical lectures. Observation of medical cases, and practical reading*, to be a chief occupation through this year. The student may pass it at Edinburgh, at least from October to July.

"Fifth and sixth years. *Close attendance on hospitals with practical reading and lectures*, at Paris and Vienna if accessible; otherwise in London. During the autumn of this or the preceding year, some time, if possible, to be employed in attending military hospitals, especially in the field.

"During the summers, *oral instruction, as it best offers, in other branches of natural history besides botany, in natural philosophy, and in the speculative sciences*, if in these last any lectures shou'd promise more than books. From one or the other, *the acquisition of as many facts as possible concerning the mental operations*, should be considered as an essential part of the stock of the knowledge necessary to the physician."

That it would be very desirable for every physician to be employed at least six years in studying his profession, before he commences the practice of it, cannot be denied; we may likewise add that he should be well acquainted with most of the modern languages, in order that he may profit by the discoveries made in other countries. A competent knowledge of the Greek and Latin is absolutely necessary. We must, however, contrast the expense to be incurred by such an education, with the probable chance of remuneration. Undoubtedly great prizes may be obtained in the medical as well as in other professions, but the number of these prizes is comparatively trifling. In London there may be men who derive a splendid income from the practice of medicine, and there may be twenty provincial towns where a physician may acquire from five hundred to two thousand pounds a year. But how many parts of England are there in which a man may reside all his life, and never hope to obtain by his utmost exertions more than will enable him to support a very small family, without the least chance of accumulating any thing for the support of that family after his decease. It is but too true that there is a number of illiterate and ill-informed men, who under the title of physicians do a great deal of harm in different parts of the kingdom; but how is it proposed to remedy this evil? By rendering it impossible for any man to practise at all who cannot afford to spend some thousands upon his education. It is not to be supposed that any person educated upon the plan which Dr. Beddoes proposes, will consent to settle in a situation where there is no chance of obtaining more than a bare maintenance; and is it not better to have even an Edinburgh graduate than a mere compounder of drugs? The question as now agitated is a very simple one. Is it more desirable to have a few perfectly educated *gentlemen,* who under the title of physicians shall practise in the principal parts of the island (and most assuredly no where else,) than to permit a number of needy Scotchmen, who have resided in some sixth flat in the old town of Edinburgh for three years, starving their bodies in order to stuff their minds, to settle in parts of England where men more expensively educated would never consent to reside? True it is that some of these same needy Scots will make their way into many of the best situations in the country, but their success is the result either of persevering industry or superior attainments. It may seem a singular statement, but it it is nevertheless a true one, that it would be by no means desirable to adopt any regulation which would considerably diminish the number of medical men in the country. Every city in the kingdom is overstocked with physicians, but were this not the case the smaller towns would be left destitute of any thing like well educated practitioners; and were it not for the overflowings of Scotland our army and navy, our East and West Indian possessions would be deprived of medical assistance. It is not to be expected that a man who could afford to pass six years at an university, will consent to settle in a place where he could never hope to make more than three hundred pounds a year. Yet there are many situations both in England and Scotland where an Edinburgh or Glasgow graduate will be satisfied to reside with no brighter prospects, and where, though not qualified to teach the healing art, he may yet save many valuable lives which would otherwise be lost.

It is not by increasing the expense of education that we can hope to advance the science of medicine; we shall only diminish the number of competitors without increasing the zeal of the candidates for public favour. We are far from wishing to discourage any attempt that may be made to reform the abuses which exist in the medical world, but in our attempts to reform let us not be precipitate. Many of Dr. Beddoes's hints are valuable: among others we think that the plan of erecting a Medical College in London merits the attention of government. No spot in Europe can be better calculated for the study of medicine than London, and nothing could conduce more to the improvement of science than an university liberally endowed, in the metropolis. We say liberally endowed, because the object ought to be to render education as cheap as possible, in order to insure a sufficient number of well educated pactitioners for every part of the kingdom, for our army, our navy, and our colonies. Why should there not be medical* as well as clerical fellowships, and honours and rewards conferred upon those who excel in medicine as well as mathematics. The necessity of reforming the Scotch colleges is obvious, but we do not agree entirely with Dr. Beddoes in his abuse of Edinburgh, which with all its faults, still possesses many advantages. The spirit of industry and the eagerness for obtaining information among the students, perhaps not to be equalled in any other university, is to be attributed in a great measure to the cheapness of education there. Few young men of fortune graduate at Edinburgh, consequently few can afford to be idle. Several improvements it must be confessed might be made in the Edinburgh school. The professorships should cease to be hereditary; the candidates for degrees should be examined every year, rather than at the expiration of three years; and no man should be allowed to matriculate unless he be found to have made some proficiency in those branches of his profession to which he ought to have attended.

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* We know there are medical fellowships in the English Universities, but they are very few in number and may be obtained by men ignorant even of the rudiments of medicine.

On the subject of quackery some interesting remarks occur, and the following story introduced to prove the existence of this art among regularly bred practitioners is worth extracting,

"One of the princesses being taken ill, and Dr. Gisborne in attendance, her royal highness inquired of the doctor if she might not indulge in the use of a little ice cream, as she thought it would greatly refresh her. Dr. G. who never contradicted his royal patients, answered that he '*entirely agreed with her royal highness*;' and the ice was accordingly provided. His Majesty, visiting the chamber and observing the glass with some of the ice still remaining in it, seemed alarmed, on the supposition that it might be improper; but her royal highness assured him that she had the doctor's permission for what she had done. His Majesty ordered the doctor into his presence, and observing to him that he had never heard of ice being recommended in such cases before, expressed his apprehension that it was on some new system. The doctor seemed at first a little confounded, but quickly recovering himself, replied, '*Oh no, please your Majesty, it may be allowed provided it be taken warm.*' '*Oh well, well, doctor, very well, very well, warm ice, warm ice.*' His Majesty seemed to be much entertained, and for some time afterwards always took the opportunity of asking those he was accustomed to meet, whether they had heard of Dr. G.'s new system of prescribing warm ice."

The pamphlet is full of amusing anecdotes, but it is evidently the production of a man discontented with all his brethren, and more ready to point out defects than to bestow praise, even where it is most deserved.

ART. II. *Remarks on the Purulent Ophthalmia, which has lately been Epidemical in this Country.* By James Ware, Surgeon, F.R.S. 8vo. pp. 57. 2s. London: Mawman, 1808.

EPIDEMIC or contagious ophthalmiæ have occurred in different parts of the world and at different periods of time. Fracastorius in his chapter *de morbis contagiosis* mentions an ophthalmia which occurred about his time in Italy. The ophthalmia which afflicted our army in Egypt is notorious. Dr. Vetch has recorded an ophthalmia which has appeared among some of our troops in England, and the number of green shades which are daily seen in the streets of the metropolis, affords sufficient evidence that maladies of a similar nature are now afflicting all orders of people.

The common forms of ophthalmia are at this time much more frequent than usual: among the rest, there is one which is marked by peculiar features; and of this the symptoms and the treatment are the subject of the present pamphlet. It is our intention to do little more than to extract or to condense the practical matter of this dissertation. Mr. Ware is one of those writers whom we read not with the expectation of finding exercise for our critical, but instruction for our practical duties.

The disease which is the subject of this pamphlet is very similar to the purulent eye of children, and from the great effusion of pus which attends the complaint, is denominated by our author the purulent ophthalmia. We extract the description of the symptoms.

"In adult persons the violence of the inflammation that accompanies the discharge is extremely different. In some the appearance of the eye varies little from that which it assumes in those cases of the ophthalmia that are produced by a common cold; but in the greater number the tunica conjunctiva, that covers both the inside of the eyelid and the globe of the eye, not only becomes universally red, but quickly swells to a considerable thickness; its membraneous appearance being destroyed, and its projection causing the cornea to appear depressed and sunk in the globe. The swelling of that part of the conjunctiva which lines the inside of the eyelid is sometimes so considerable that it protrudes beyond the lid to a considerable extent, and entirely conceals the eye.

"In some the pain produced by the inflammation resembles that of dust between the eye and the eyelid; in others it is accompanied with a sense of heat and weight; in others there is a deep seated intense pain in the ball of the eye; and in not a few the pain in the eye is accompanied with a distressing pain either in the whole or some particular part of the head. The pain is not in proportion to the quantity of discharge; since, when the latter has been profuse I have, in some instances, observed that the former has been far from considerable."

The purulent is very similar to the gonorrhæal ophthalmia. The purulent eye most commonly occurs in the children of those women who have had an acrimonious discharge from the vagina at the time of delivery; and the purulent ophthalmia of adults is very generally found connected with some gonorrhœal affection. In public schools the disease obviously spreads in consequence of the indiscriminate use of basons and towels among the children. Hence Mr. Ware believes that the purulent ophthalmia arises from the direct application of some poisonous matter to the eye.

This disease is to be distinguished from the venereal ophthalmia by attending to the symptoms of both: those of the former have already been detailed. The latter is marked by great aggravation of pain during the night; by the internal parts of the eye being much affected; by the pupil being contracted and little moveable, and by the iris being green and sometimes having small pustules on its surface. In the purulent ophthalmia the cornea and tunica conjunctiva are chiefly affected.

The erysipelatous inflammation of the eye, from which it must also be distinguished, is chiefly marked by general tumefaction of the eye-lids; by watery discharge from the eyes sometimes mixed with a *small* quantity of pus-like matter which seems to be formed by the ciliary glands, and not as in purulent ophthalmia by the tunica conjunctiva; by a greater impatience of light; and a more extensive spreading of the inflammation over the face, sometimes affecting other parts of the body.

When the purulent ophthalmia is attended by very violent symptoms, the common evacuating remedies often fail to impede its progress: general blood-letting from sixteen to sixty ounces has been used with great success in the army. Mr. Ware however, recommends it to be used in a less degree, and when the disease occurs in debilitated persons (which is not rarely the case,) he prefers opening the vein on the side of the nose, or the application of leeches to the temples, or the scarification of the inner surface of the eye-lids.

"This last operation is easy to be performed; and, if it be done well, it gives very little pain, and will not be followed by any irritation of the eye. The lancet which is used should be in the best order, and should be carried along the inside of the lower lid, parallel to its edge, and not far distant from it. The advantage of scarifying the lid near its edge, in preference to performing the operation either on the ball of the eye, or further back on the lid, is derived from the superior tension which the tunica conjunctiva has in this part, in consequence of the cartilage that lies behind it; and if the operation be performed here, and, as is above recommended, with a sharp lancet, thirty or forty drops, and sometimes a larger quantity, of blood may be taken away with almost immediate advantage. I beg leave, however, to offer a caution against a mode of scarifying, which has sometimes been adopted in these cases, by pricking the eye-lid, ten, twenty, or thirty times in quick succession; a mode which, besides the pain that it occasions, is liable to increase the irritation, and, to say the least, is certainly unnecessary. The lancet need never be applied more than twice, and rarely more than once; and perhaps it will give less pain, if the incision be made by drawing the edge of the instrument along the inflamed membrane, from the point towards the handle, instead of using the direct point for this purpose. The issue of the blood from the orifices, may be assisted by gently everting the lid with the end of the finger; and it is more useful to take off the finger occasionally, and then to apply it again, and thus renew the eversion, than to continue the finger steadily on the lid. Some apply warm water, as is done after the application of leeches, in order to promote the bleeding, but it does not appear to me to be of much use for this purpose."

After bleeding, a large blister should be applied to the head or back; anodynes should be exhibited according to the degree of pain; the bowels should be kept open, and the antiphlogistic regimen strictly observed.

A remedy however, on which Mr. Ware lays great stress is the aqua camphorata of Bates; which in his former publications he has recommended so strongly, and, we may add, so justly, in the purulent eye of infants. The following is the formula which he uses;

℞ Cupri Vitriolati,
Boli Armeni, aa gr. viij.
Camphoræ, gr. ij.
Misce et affunde.
Aquæ Bullientis ℥viij.

This lotion is directed to be injected by means of a blunt syringe inserted between the eye-lids; by this means the purulent matter which forms with great rapidity, is effectually washed from the whole surface of the eye-lids. Mr. Ware sometimes uses it twice, and rarely less than once in an hour, diminishing its strength and frequency with the diminution of the symptoms. When, however, the pain of the eye and the inflammation of the tunica conjunctiva are great, he recommends it to be used weaker, to be made lukewarm, and to be injected less frequently. Sometimes it may be alternated with tepid water, and conjoined with anodyne fomentations.

Another peculiarity of practice to which Mr. Ware wishes to call the public attention relates to the treatment of those cases where the bursting of the cornea is threatened. When the cornea is ruptured spontaneously, the inflammation suddenly subsides. Our author takes the hint and applies it to practice. When the rupture of the cornea is inevitable, the only question is, shall the opening be made by Nature or by the surgeon? The former will make it clumsily, and in such part of the cornea as afterwards to impede the passage of light; the latter may make it in such a situation that no impediment of this kind shall arise. Mr. Ware, therefore, recommends puncturing the cornea in those cases in which would it otherwise burst. This is discoverable by the cornea losing its transparency, and a white rim forming around its circumference.

"The puncture may be made with great safety by a careful person with a common lancet; or, which perhaps is preferable to either of the two, with a sharp pointed couching needle, having a blade somewhat wider than this instrument usually has, and a groove in its middle continued all the way from the handle to the point. The only direction necessary to be observed in performing the operation is to introduce the point of the instrument about the tenth part of an inch anterior to the connexion of the cornea with the sclerotica, and to push it gently forward, parallel to the plane of the iris, until the aqueous humour make its escape. If the grooved couching-needle be employed, its evacuation necessarily takes place as soon as the groove penetrates into the anterior chamber; but if a cataract-knife or a lancet be used, and these possess the shape of a wedge (which in the former instrument is peculiarly desirable) it may be necessary to turn the blade a little on its axis after it has been introduced, in order to give the aqueous humour an opportunity of escaping; and when this has been accomplished, the instrument should immediately be withdrawn, to avoid wounding the iris."

The substance of this useful pamphlet we lay before our readers without a comment, and take no credit to ourselves, but that of having comprized all its important—we might say, *all* its information, in a very small portion of our review. The art of condensing so as to lose nothing—of telling equally well in a few pages, what is told in as many chapters, is a very useful but not a very common art: we shall therefore cultivate it as an instrument most valuable in our undertaking. Some compositions require only simple concentration, others must be purified by critical filtration: when they are both defæcated and undiluted, which must happen very seldom indeed,

"Othello's occupation's gone!"

ART. III. *Essays on the Diseases of Children with Cases and Dissections, Volume I. containing—Essay I. Of Cynanche Trachealis, or Croup.—Essay II. Of the Bowel Complaints more immediately connected with the Biliary Secretion.* By John Cheyne, M. D. Fellow of the Royal College of Surgeons of Edinburgh. 4to. pp. 80 and 72. 15s. and 12s. 6d. Edinburgh, Mundell and Son; London, Longman and Rees, 1800 and 1801.

An Account of the Diseases most incident to Children; to which is added an Essay on Nursing, with a particular View to Infants brought up by Hand, also a short Account of the Dispensary for the Infant Poor. By the late George Armstrong, M. D. A new Edition, with many additional Notes. By A. P. Buchan, M. D. pp. 242. 7s. London, Cadell and Davies, 1808.

THAT the diseases of children are little attended to and less understood, is a complaint which has been repeated by almost every writer on the subject for more than a century.

At present we have surely little cause to complain of deficient attention; for within a short space we have seen at least four systematic works concerning them, some of which have been repeatedly edited; and numerous partial essays on particular infantile disorders. Many important improvements in the knowledge and treatment of this class of affections, have unquestionably resulted from these recent inquiries. Among the most prominent of these we may notice the essay of Whytt on the Dropsy of the Brain; that of Quin, who proceeded further in the same investigation, and perhaps that of Dobson, who suggested the present popular method of treatment; the treatise of Home on the Croup, which doubtless gave birth to that of Michaelis; the radical change lately made in the constitutional treatment of early scrophula*; the use of the cold affusion, especially in scarlatina; and the discovery of the connexion of epilepsy with the period of second dentition. Though we may admit therefore, the correctness of Harris, who spoke on this subject as "de re adeo hactenus desiderata, et cujus plenam notitiam (ne quid gravius dicam) medici omni laude dignissimi vix ac ne vix quidem sunt assequuti;" yet we cannot allow to Dr. Cheyne that "still this department is strangely overlooked." We are willing to concede to him, however, that our advances in knowledge have been by no means proportionate to the number and size of our books, of which his own may be cited as an an ample proof.

Many causes impede the advance of medical science in this direction; of these the dissimilarity of the affections of infancy and riper age, the impossibility of gaining information from the patient's sensations, the suddenness with which some of the more violent disorders occur, precluding previous attention to minute, though important circumstances, and the difficulty of procuring dissections, are among the most important. But the chief we conceive to be the almost exclusive appropriation of those diseases to a particular class of the profession. "Populus," said Harris in 1705, "(ita usu iniquo comparatum est) pharmacopolarum aut muliercularum peritiam amat aliquamdiu experiri, dum in extremo discrimine versentur, priusquam ad medicos doctos confugiat;" and Heberden in 1807 talks of "the illiterate and ignorant people, who have at all times been too apt to suppose themselves qualified to undertake this branch of medicine." The arts may benefit by subdivision, but it is otherwise with science. This is seldom partially progressive, and is rarely advanced but by those who with comprehensive views of the whole, have power to devote their leisure to those parts to which inclination may lead them. Whatever may be the talents and early education of the practitioners in midwifery, (and when we remember those who lately stood at the head of that department, and those who now do so, we cannot think lightly of either,) yet they must want cultivation in maturer years. More demands are confessedly made on their time than on that of other professors of physic. From their interrupted hours of leisure and rest we cannot justly expect a share of professional improvement proportioned to their numbers. Medicine in general, though its recent improvements have been great, has not advanced equally with other sciences; in part, doubtless, because its professors rarely receive that general and strict scientific education which their important duties require. Much good, we are convinced, would have accrued to medicine, if such a rigid system of tuition, as well as the cultivation of classical taste, had chastised the prurient imaginations of some late and some existing distinguished authors.

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* We allude to the substitution of animal for vegetable regimen, and not to the specific so highly extolled by Beddoes. Of the muriate of lime we have made repeated and persevering trials, but we cannot add with decided advantage. Our trials of it, we trust, have been fair; for, however the versatile vehemence of Beddoes may have lessened our confidence in his sagacity, we hold in high esteem the fertility and ingenuity of his mind, and are anxious to find it proved by general testimony that he has not been thus rarely gifted in vain. For this proof, however, we are destined still to hope, the profession not having yet received, as we think, any important favours from his hands.

It is usual with systematic writers on these diseases, to assert that the difficulties in their treatment are rather imaginary than real; and that a little practice will enable a man of competent understanding to manage them with equal, if not greater facility than those of maturity. "Quisquis, igitur, infantum minimorum symptomata, quæ manifesto se produnt, diligenter expendit; qui crasin illorum molliculam, simplicissimam diætam attente considerabit, non ita arduum et difficile pensum hoc fore comperiet, ut falsa imaginatione ductus, prius existamabat. Etenim asserere non verebor morbos illius ætatis genere paucissimos esse, et gradu tantummodo differe; imo curationem puerorum multo tutiorem ac faciliorem quam virorum et mulierum*." Nearly the same statements have been made by more recent writers†. To such assertions we can by no means give an unqualified assent. Practitioners much conversant with this department, may no doubt acquire as by attendance on adults a readiness in deciding, and a knack in prescribing, which may greatly add to their comfort, and facilitate the course of their business. Doubtless sedulous attention may really remove much of the obscurity which hangs over these subjects: yet careful and anxious experience has convinced us that, in despite of all who have written, and the ease in practice of which they boast, our ignorance of many of the important diseases of infants, whether of the head, the breast or the abdomen, continues to be great. Every one who has been much called to attend children in a populous district, must have repeatedly witnessed deaths in cases which he had thought slight, and recoveries in those which he had thought hopeless; and must have confessed, if he have been candid with himself, that his reasonings, formed even after much reading and reflection, have been set at nought by such events.

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* De Morbis acutis Infantum auctore Gualtero Harris.

† V. g. Armstrong. Underwood.

Dr. Cheyne who is a surgeon in Leith, informs us that he has been for some time engaged in "an extensive range of practice;" that he has paid peculiar attention to this branch of his profession; and that he has collected a number of cases and observations, which, being satisfied that these diseases are "ill understood and superficially treated," he has arranged systematically, and means to give to the public in separate essays. Of these, two only have appeared, which does not surprise us when we remember Dr. Underwood's formidable catalogue of infantile ailments, and consider how the public are likely to receive each of these items, spread out upon a thin, thick-leaved, ornamented quarto.

In the first essay Dr. C. treats of croup, concerning which we have possessed for at least fifty years, all the knowledge which his book contains. There are points indeed connected with the subject, which still want elucidation, but these the present author has not supplied. The points on which we are at present solicitous for information respect the use of mercury, and the distinction between the inflammatory and spasmodic species of the disease. On the former subject scarcely any thing is said; and the latter is thrown hastily into an appendix of two pages.

Were we inclined to cavil with the author we might cite his definition, of which he makes "an effusion which becomes a tubular membrane," an essential part. Now we believe that in many truly inflammatory cases the disease is arrested so early as to prevent the formation of this membrane; and that in instances where more spasm and less inflammation prevail, the membrane is never formed at all. Dr. C. informs us that the croup occurs more frequently in some families than in others; that it appears chiefly in low situations, and in damp cold weather; and that it sometimes attacks the same person frequently.

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He describes the usual symptoms of the more violent inflammatory affection; and says that bleeding, blistering, the warm bath, emetics, purgatives, and sudorifics are the chief remedies to be used. There is nothing in all this but what may be found in any of our modern encyclopædias, nothing for the re-perusal of which we can advise our readers to purchase a quarto. We believe that in England the plan of bleeding is not carried by any means so far as is recommended by our author. In Scotland we know that, even in populous towns, bleeding is more frequently practised, and better borne than with us. Yet the present professor of the practice of physic in Edinburgh, who has no weak fears about blood-letting in general, cautions strongly against the free use of the lancet in infants, either in this or any other diseases, as they are apt to be materially injured by comparatively small losses of blood. When to this we add the general testimony of Harris—"aperte constat eam (venæsectionem) non esse medicinam puerorum naturæ consentaneam, neque magis huic tenerrimæ quam decrepitæ ætati accommodatam*;" and the particular assertion of Baillie that "croup is rarely cured by the means which are known to remove common inflammation †;" we shall probably pause before we adopt the sanguinary practice of our author. Dr. C. strenuously inculcates the necessity of general and local blood-letting, as that part of the treatment on which he most confidently relies; and the value of his book consists in a series of cases in which that practice and its effects are detailed. He generally takes the blood from the external jugular vein, and considers from three to five ounces a full bleeding for a child under five years of age.

In the appendix a case is given arising from similar causes with croup, but which is more spasmodic and less inflammatory than the usual affection. This Dr. C. considers as a distinct disease, which he calls acute asthma, and treats with antispasmodics. Now we maintain that these two affections are not separated from each other by such broad limits as our author supposes. The extreme instances, when placed in contrast, may certainly appear widely dissimilar, yet the one is not purely inflammatory nor the other purely spasmodic. In each there is cough, but in the one it is incessant, in the other slight. In each there are remissions, but in the one they are little conspicuous, in the other obvious: in each there is fever, but in the one it is vehement in the other more moderate. We believe that the more frequent varieties of croup in England are intermediate between the examples given by Dr. Cheyne; and that in a great number of instances in which the propriety of copious and general bleeding is highly problematical, the active use of mercury, as recommended on high authority, is likely to be greatly beneficial. This form of the disease, however, is not that which Dr. C. has generally seen, and he therefore expresses an unwillingness to relinquish his plan of depletion, in order to give calomel that fair and full trial from which alone an estimate of its powers can be drawn.

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* De Morbis acutis Infantum, p. 58. † Morbid Anatomy, 3d Ed. p. 118.

We find assertions that croup "generally attacks the most robust and ruddy children;" that it "legitimately belongs to the order of inflammations; an order of diseases of which the general treatment is peculiarly applicable to the disease under review;" and yet that "debility of the trachea is the predisposing cause of croup." We notice this, not for the purpose of fatiguing our readers by any intricate disquisitions concerning the proximate cause of inflammation, but to show how men of fact and practice will now and then boldly slip in a conjecture; and how necessary therefore it is for their readers to separate that which has been seen from that which has been only supposed.

We regard the second Essay as of much more value than the first inasmuch as it relates to a class of diseases of which the investigation is more wanted. The histories indeed of almost all the abdominal diseases have been pretty fully detailed. Those however who have described them, from Harris to Hamilton, have generally adopted some sweeping conclusion respecting their cause, precluding that accurate inquiry which we think essential to the exhibition of adequate means of relief. The first disorder noticed in this second Essay is one to which too little attention has been paid. Jaundice in infants, similar to some species of that affection in adults, not unfrequently occurs; yet by Underwood, Heberden[*], and others, who as authors of compilations should give us a full statement of our actual knowledge, it has been insufficiently described. By Armstrong a fatal case is casually given in a note, unaccompanied by any account of the nature or treatment of the disease. Nevertheless Cheyne has given extreme cases, similar to those published by Pearson, which may tend to mislead. The most common variety of infantile jaundice is unquestionably slight. A few days after birth, the skin becomes tinged with a dirty yellow, the infant is listless, flatulence occurs, and digestion is impaired. Yet for the most part these symptoms soon spontaneously subside; the appetite increases, the bowels recover their functions, and the skin gradually clears. Even where the disorder is more serious and permanent, we by no means regard it as so fatal as it is represented by Dr. C. The serious cases which we have most frequently seen, though attended with a tinged skin, clayey stools, listlessness, and not unfrequently an increased size and firmness of the liver, have usually continued, with intervals and recurrence, for months or even for years, and generally without a fatal consequence. When they cease they often leave great delicacy of constitution, and especially of the digestive organs. Dr. C. gives a different statement. Speaking in general terms, of the more severe affection, he says, "I believe it to be an original and incurable mal-conformation of the liver;" and, "I doubt much whether any thing beyond assisting the breast-milk by a gently laxative medicine ought to be attempted, or perhaps frictions of the belly might be useful." In those protracted cases which we have most frequently seen, we have thought occasional purges, the warm bath, and small repeated doses of calomel of considerable use. All the cases cited by Dr. C. were fatal. He believes the obstruction to arise from "an impermeable thickening of the beginnings of the hepatic duct or as they are called, the pori biliarii." His own dissection may countenance this opinion, for the gall-bladder was empty and collapsed, and the ducts permeable. But in the dissection given by Pearson the gall-bladder was nearly filled with bile of a deep yellow colour; the ducts in this case also being permeable. In the dissection by Armstrong the same thing occurred, but as this note gives, in a short space, a distinct account of one of the worst forms of this obscure affection, we shall quote it at length. "In a child which I opened a few years ago that died at the age of ten months, of obstructions in its bowels which occasioned want of digestion, and in consequence thereof, a marasmus; the gall in the vesica fellea was as thick and ropy as a strong mucilage of quince seeds, and of a deep saffron colour. The child was never thriving from the birth, had been ill a month before I saw him, and was very much reduced. He had a slow fever almost constantly upon him, and his complexion was very sallow, like a person's in the beginning of the jaundice. His urine was high-coloured, his stools whitish, very tenacious, and offensive to the smell: and he was generally inclined to be costive except when laxatives were given him. When opened the abdominal viscera appeared all very sound, nor could any thing be discovered to account for his complaints except the above viscid quality of the bile in the gall-bladder, which had tinged the neighbouring parts of a deep orange colour. The father of this child was a native of the West-Indies, and subject to bilious complaints." The chief part of Dr. C.'s second Essay is occupied with the description of what he calls *atrophia ablactatorum*; an affection which he considers that he has been the first to investigate accurately; and for which he has pointed out a remedy hitherto used only casually and vaguely. Bowel complaints from birth to puberty are, we are convinced, imperfectly understood; we are therefore peculiarly gratified by any attempt to elucidate by dissections any part of the subject. We cannot however assent to the assertion that this disorder "is overlooked by those physicians who have made these diseases their study." Every physician, we believe, who has treated of infantile diseases, has treated of diarrhæa with griping, green or otherwise unnaturally coloured stools, and with more or less of indigestion and marasmus. Each too has commonly referred these complaints to improper diet, to cold or to want of due exercise and air*. That improper diet will be more likely to produce these effects after sudden weaning than at any other time, we are wiling to admit, and therefore have no great objection to our author's new name: but we have seen this set of symptoms so frequently both long before and after weaning, that we must protest against their being regarded as forming at different times different diseases. The disorder described at p. 12, et seq. we regard as the same with that which is treated of through the rest of the essay. The disease is thus described: "The first symptom is a purging with griping pain, in which the dejections are usually of a green colour. When this purging is neglected, and after continuing for some time there is added a retching with or without vomiting; when accompanied by vomiting the matter brought up is frequently coloured with bile. These increased and painful actions of the alimentary canal produce a loathing of every kind of food, and naturally are attended with emaciation and softness of the flesh, with restlessness, thirst, and fever." After some weeks a hectic blush, emaciation to a great extent, unceasing griping pain, and constant peevishness come on. In the progress of the disease the stools are "sometimes of a natural colour, at other times slimy and ash-coloured, and sometimes lienteric." "Towards the end of the disease the extremities swell, and the child becomes exceedingly drowsy." "The disease seldom proves fatal before the sixth or seventh week; and in this short time I have seen the finest children miserably wasted, I have seen, though rarely, a child recovered after the disease had continued three or four months; and again I have seen the disease cut short by death in the second, third, or fourth week before it had reached the acme; the sudden termination having been occasioned by an incessant vomiting and purging, or by convulsions from the immense irritation in the bowels."

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[*] Dr. Heberden we think, has from some motive or other, chosen to publish a compilation respecting diseases with which he is not familiar. He professes indeed to add nothing new, but to "bring the whole into one view, unencumbered by the unscientific or absurd observations of illiterate and ignorant people." A decent education and common sense have enabled him to clear his subject from absurdities in *reasoning*; but a certain practical knowledge, in which from internal evidence, we think him deficient, is essential to one who proposes to give a judicious selection of *facts*.

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* Heberden. Armstrong.

When Dr. C. began practice he found this disease not understood; some spoke of it as arising from teething, and others from scrophulous affections of the mesenteric glands. Being little confident in the accuracy of these assertions, he resorted to dissection, and discovered that the mesenteric affection was by no means essential to the disease; that frequent spasmodic contractions, with occasional intro-susception took place in the intestines, and that the liver and bile were changed in appearance. Hence he concludes that the disease consists in an irritation of the bowels by improper food, by which the secretion of bile is vitiated, and the mesenteric glands frequently diseased. For the cure of this affection he recommends with great confidence, small and repeated doses of mercury as the chief remedy on which dependence can be placed.

We have read this part of the essay with much satisfaction; and regard with pleasure the author's sedulous investigation of his subject. Benefit, we trust, will arise from his labours, though probably not to the extent which he expects. In the early periods of the disease, due attention to diet, to the state of the skin, and especially to the just action of the bowels will often prove effectual. In the latter stages, when the structure of the chylopoietic viscera is already changed, we fear that no remedy will be of much avail. In the intermediate degrees, calomel will, doubtless, be our best agent: yet, we must add, that even here it will be found much more frequently to fail, than we should have expected, had we relied implicitly on the authority of Dr. Cheyne.

Dr. Armstrong, in his chapter on watery gripes, (essentially the same disease we conceive, with Dr. C.'s more decorous atrophia ablactatorum,) has given some useful information. Whether these disorders with which the bowels of children are so often affected, be varieties in kind or in degree, the varieties themselves are great. The train of symptoms described by these authors is, as we have said, the same:—they are severe griping, vomiting, purging; the stools green, brown, or of various colours, and fœtid; hectic and emaciation. These are connected with various states of the abdominal viscera, from the simple excitation of crapula, to spasms, diseased secretions of the intestines, inflammation of their coats, ulceration, diseased action and altered structure of the liver. Dr. C. has traced some of these morbid appearances; and those who will point out more precisely the train of minute symptoms with which each is accompanied, will have proceeded far towards the discovery of an appropriate method of treatment. At present our modes of practice are clearly too compendious. Dr. A. for example, is little less warm in his commendations of antimonial emetics, than Dr. C. is in his plaudits of mercury. Dr. A. however, has given some cases and dissections, which strongly show the necessity of discrimination. Having already exceeded our due limits we cannot transcribe them at length. In one child, who died with the usual symptoms, he found no morbid appearance, except an inflammation in one kidney from gravel, when no such affection had been suspected; in others he has found the coats of the intestines tender as from maceration; in some he has found them inflamed; in some he has discovered collections of matter between the coats of the intestines, and adhesions of the different viscera to each other; in others, he has found only the remains of spasm. These dissections we cite to prove the necessity of further scrutiny; they are given by the author merely as excuses discovered after death, for the failure of his specific antimony. In some cases where much fever, heat, thirst, quick pulse, and dry skin occur, he advises bleeding, even infants at the breast. In some modified inflammations, as of the bowels in dysentery, of the liver in hepatitis, of the joints in rheumatism, and perhaps of the trachea in croup, mercury is an highly efficient remedy; how far it may be so, in those states of inflammation which preceded death in many of Dr. Armstrong's cases, we are not yet entitled to decide.

Concerning the other parts of Dr. A.'s work, since it has been long known*, the public cannot be supposed to wish for copious or detailed information from a review. Nothing, that we can detect, has been added to its value by the present editor; unless we can regard as improvements his wish to throw salt into all the food of infants, their milk, poor things! not excepted; his discovery that spitting and speaking are acquired faculties; and his proposal to accustom children when in health to take frequently something nauseous, in order that if they should chance to be sick, they may swallow his drugs at the word of command. Dr. A. we think has had more sense and more vigour, than most of the writers on this class of diseases. In his chapter on convulsions however, we meet only with this unpleasant singularity: he says that the idiophathic species "certainly require a different treatment" from the more common forms; and that they "are much harder to manage." Yet for the management of these hard cases he has not given one word of direction. This omission Dr. Buchan seems with us to have thought important; for he supplies it by a long note to tell us that "when they do occur the child's feet must be well warmed at the fire; and the region of the stomach well rubbed with a warm hand." He adds "the most proper medicines are a few grains of rhubarb in dill water, a little of the musk julep, or some drops of the aq. ammon. comp."

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* The first edition was published in 1767.

In the chapters on hectic fever from teething, and on worms, is contained an account of a disease, which the author seems to regard as the chief merit of his work. The affections thus separately described we regard as the same with each other, and also the same with Hamilton's marasmus. An irregular but generally costive state of the bowels, unnatural stools, tumid belly, fœtid breath, irregular fever, and tendency to spasm are the general signs of this disorder. As Hamilton in his chapter on this subject, has been thought to hold singular opinions, and to inculcate a bold novelty of practice, we shall extract what Dr. A. said long ago on the subject. "Though worms by many people are reckoned one of the pricipal sources of children's complaints, yet I cannot help thinking, that worm cases more rarely occur than is commonly imagined; for I am very well assured that the common symptoms of worms, such as picking the nose, grinding the teeth in the sleep, starting and calling out while asleep, sleeping with the eyes half open, a wan complexion, a stinking breath, pains in the stomach and bowels, a want of appetite, or, on the contrary, a perpetual craving for food, a depraved appetite, &c. I say I am well assured that all these symptoms are frequently produced from a foulness of the primæ viæ when there are no worms in the case. I have good reason to say so, because I have had numbers of children with the above symptoms under my care, who by the use of proper medicines for cleansing the bowels, have got rid of them all, without ever having been observed to void one single worm, though while their purging medicines were operating, the stools were carefully inspected. Nay, I may venture to say, that of the whole number of children brought to the dispensary who from the apparent symptoms are supposed to be troubled with worms there is not one in ten that has ever voided any, nor do any make their appearance during the whole time of the cure."

Though we give a general assent to this doctrine and practice, yet we feel it our duty to state, that his treatment fails in a great variety of instances, at least in a much greater variety than we should have supposed from the accounts of the authors, to whom we have referred. We have repeatedly seen it employed early, and continued sedulously without effect. To what is this to be attributed? We presume to the fact, that, as in those bowel-complaints which we have already discussed, the same set of symptoms is connected with different internal changes—with changes of function, and changes of structure. Hamilton is too compendious in his principles to be much addicted to dissection. Armstrong is more discriminating, and has given some cases in which marks of inflammation and adhesion were discovered after death; it is known too, that the glands of the mesentery are often affected. In what proportion of cases these effects may take place we know not, for we have never been able to detect in any medical author, any precise means of distinguishing these affections. It is true indeed that each author on the subject tells us—these are the symptoms of worms, and these are the symptoms of teething hectic, and these are the symptoms of tabes mesenterica: yet we maintain, that there is no real difference, scarcely an apparent one, even on paper, between their accounts. The precise Pemberton says, "when there is a disease in the mesenteric glands, the patient *usually* complains of a lancinating pain, deep seated within the abdomen;" this is the chief mark of distinction between the disease of action and the diseased structure, yet if we wish to apply it in practice, how shall we do so, when we learn as the sentence proceeds, that this pain "is of very short duration, and resembles more the common gripes, than it does that pain which attends inflammation of internal parts." In short, we must recommend in these cases as we have done before, a reference to multiplied dissections, for these alone can guide our opinions and our practice in obstinate and difficult cases. Dissections of children, however, are perhaps more frequently opposed than even those of adults; till they can be obtained we advise our readers not to expect or promise too much in these cases, from the power of purges. Various other subjects are treated of in Dr. Armstrong's work, but on these we shall not dwell. The volume concludes with an account of a Dispensary for the relief of infant poor, established in 1769, and closed in 1781 on account of Dr. A.'s declining health. It has not been revived, which we think a subject of serious professional as well as public regret, for the annual number latterly relieved there exceeded 4000.

As Dr. A. professes to write only from experience, and to be desirous only to be heard when he has something to say, his book is not large, nor his articles numerous. Yet we think that he has acquitted himself fairly, so far as he has gone. His work, without giving proof of any superior talents, indicates attention and good sense, qualities of sterling worth, not often to be met with amidst the present extended currency of paper.

If Dr. Cheyne will prosecute his dissections with equal care, and will publish an account of them in a modest form, we shall receive his works with pleasure. To such a publication he may be fully equal, for it does not require the hand of genius: if executed simply with fidelity, it cannot fail to give an accuracy to our ideas respecting the diseases of children, which they do not at present possess.

These essays are ornamented by engravings, distinguished by the usual indistinct prettiness of Mr. Charles Bell. To this gentleman, too, we would recommend a more modest style of publication. With his skill as a limner, and his anatomical knowledge, he might be useful in his generation, if, instead of attempting by lake and vermillion to give an air of gaiety and beauty to his ragged limbs and bloody entrails, he would strive to copy the simple unpretending elegance, to be found in the works of Baillie, of Cooper, and of Scarpa.

Art. IV. *An Enquiry into the Seat and Nature of Fever; as deducible from the Phenomena, Causes, and Consequences of the Disease, the Effects of Remedies, and the Appearances on Dissection. In two parts; Part I. containing the General Doctrine of Fever.* By Henry Clutterbuck, M. D. 8vo. pp. 440. 9s. London, 1807.

IN our first number we had occasion to examine an Essay on Fever by Dr. Wilson. We have now before us two fresh candidates for notice upon the same disease; and if we consider the acknowledged difficulties of the subject, and the great difference of opinion between three authors thus treading the same ground in rapid succession, we can have little reasonable hope, that the matter will be brought to any speedy and satisfactory issue. But as it is said that *in a multitude of counsellors there is safety*, so in the conflict of opinions, useful truths may be elicited; and although in an undertaking which presents so many and such varying obstacles, no individual explorer may perhaps ever attain the shortest and the safest track possible—yet the labours of each, like voyages of discovery, will serve to improve our general stock of knowledge and thereby at least *practically facilitate* the progress of those who come after them:

“ *Est quadam prodire tenus, si non datur ultra.*”

The author indeed seems well aware of the magnitude of his attempt; and in his Preface justly observes,

“ It will be readily admitted that the task I have undertaken is one of no small difficulty or extent; which will serve, I may hope, to palliate the imperfect execution of it. It is a task too, that, after almost innumerable attempts, has never yet been adequately performed.”

“ I have not, however, been deterred by the ill success of former inquirers, from entering the field of speculation on so important a subject; believing as I do, that the difficulties which lie in the way, will be sooner or later surmounted, and that the most beneficial consequences will, in regard to practice, be the result of a successful investigation of it; and that even in case of failure, collateral advantages may accrue.

“The advantages to the medical practitioner of a just theory of disease, will scarcely be doubted Experience, undoubtedly, is a safe and sure guide as far as it goes; but it is too often lame and defective. It is impossible to foresee and provide adequately for the infinite variety of changes that take place in living bodies, without the aid of analogy and induction; the practice of physic without these must ever be incompetent to meet the continually varying circumstances of disease!

“ When we reflect on the remedies that have been recommended and employed in the cure of fever, we cannot but be struck with their number and variety. Every *age*, and almost every practitioner, indeed, possessed of sufficient courage to think and act for himself has had *his* favourite remedies; which have been relied upon as if they were exclusively adapted to the purpose; while the practice of others has been considered as worse than useless.”—“ Bloodletting, sweating, blistering, cordials, refrigerants, acids and alkalies, heat and cold, have all been favourites in their turns, and considered as alone deserving of confidence.

“ This contradiction exists, or has existed, with regard to every active remedy employed in the cure of fever, and occasions the greatest embarrassment to the young and inexperienced practitioner. The want of a fundamental principle to guide him in his choice of means, has been strongly felt and acknowledged; and it can only be supplied, as it appears to me, by the establishment of some consistent theory of fever, capable of explaining at once the phenomena of disease, and the effects of remedies. While we continue to look on fever as a general affection, without any peculiar and determinate seat in the body, we can never hope to discover any thing certain with regard to its intimate nature; nor to be able to lay down any precise rules for its cure.

“ How far the present doctrine (viz. *that fever consists essentially in topical inflammation of the brain or its membranes*) is calculated to lessen or remove the difficulties above complained of, it is not for me *to* determine. It is, perhaps, a presumptive argument in its favour, that it is not opposed by experience, but at once explains, and is supported by, the effects of the best established modes of cure."

Without committing ourselves to any implied acquiescence either in the truth of the author's fundamental principle or in the relevancy of the arguments brought in support of it, we may observe, that these remarks are, *in general,* pertinent and well expressed. We were, therefore, not a little surprised to find Dr. C. profess to adopt, as his rule of investigation, the following dogma of Sauvages; "*Non ex causis, nec ex sede morborum, ad eorum symptomata, sed ex symptomatibus ad sedem causasque morborum est procedendum, tutoque progredi potest medicus;*" as it may be made clearly to appear, not only from a variety of examples, but even from the affirmation of the author himself, that such a rule would, in very many instances, lead us into the most egregious errors. How often for example, might we not apply a liniment to the shoulder, to allevaite the pain arising from disease in the liver; or bleed largely, and perhaps apply a blister to the pit of the stomach, to remove the violent retching and pain of that organ, which were caused by the obscure irritation of calculous matter lodged in the kidney? Innumerable other proofs of the same might be brought forward; and perhaps the greatest improvement the practice of medicine can ever receive, will be from thoroughly investigating, and accurately applying, the *seats and causes of disease,* to elucidate and explain the (otherwise) often incomprehensible concourse and train of symptoms, or external and obvious phenomena, which constitute what is termed its nosological character. The truth is, that Dr. C. has here fallen into a palpable error, in adopting as applicable to the *practice of medicine,* a rule which belongs to *nosology* ; and has, without seeming to be aware of it, virtually corrected himself, as will appear from the following passage.

"The great advantage to be derived from an acquaintance with the primary *seats* of diseases, is our being able to administer remedies that are more especially adapted to the particular organ affected, and to make our applications more nearly or directly to the seat of mischief. This knowledge likewise tends to prevent the use of superfluous, and sometimes of injurious remedies; the natural consequence of busying ourselves about symptoms, the removal of which has either no influence on the original disease, or is only to be accomplished by means that are detrimental to it."

Dr. Clutterbuck begins his INQUIRY by a chapter of *preliminary considerations,* subdivided into three sections, which are respectively entitled, 1. *Laws of the system in health;* 2. *Of the nature of disease generally;* and 3. *Of the common division* *of diseases into universal and local.* The two first, as containing only a brief, but sufficiently clear mention of well known and generally admitted facts, we shall pass over. The third however, being the first step to the vestibule of the future edifice, and both from its position and bearing an essential one, requires notice; and here it is but fair to listen to the author's statement of the matter.

"41. Diseases have been commonly divided by authors into two principal classes, *universal* and *local;* the former comprehending such as affect the whole system; the latter certain parts only; the rest continuing to perform their functions as in health. I think it may be shewn, that there is no proper foundation for this distinction; that, strictly speaking, all diseases are in their *origin* local, or affections of some particular parts or organs, and *never* of the entire system.

"This might be inferred, indeed, *a priori*, from what has been already said. For when it is considered that every part of the system is endowed with a peculiar kind of susceptibility, rendering it liable to be affected by certain causes only [*more particularly ?*] that an increase or diminution of action in one part generally [*often*] induces a contrary mode of acting in others; and lastly that *the causes exciting diseases, all* of them produce *peculiar* or *specific* effects, it might naturally be expected, that diseases would always be partial or local at their commencement. And this conclusion will be confirmed, I apprehend, by an attentive observation of diseases themselves.

"42. It is, no doubt, true, that in the progress of many disorders, a great number of functions come to be deranged, and at times scarcely any seem to go on precisely as in health. This is particularly observed to be the case in fevers of the malignant kind; but it occurs as well in diseases that are confessedly of local origin. Thus inflammation of the brain, stomach, and some other important organs, occasion great disorder in the system, but are nevertheless *considered* as topical affections, and treated accordingly.

"43. A disease can only, in strictness, be termed general or universal, when it affects *every part* of the system *at once.* But there are evidently none such. The whole system may, indeed, be weakened, and *all* its actions be consequently diminished, as by loss of blood; but such a state, if it affect all parts equally, is not disease, though it, perhaps, strongly predisposes to it. Something more is wanted to constitute morbid action. Under such a state of general weakness, the functions *may* continue to be carried on, though less vigorously than before; and until one or more of these become deranged or interrupted, or until some uneasy sensation be induced, disease can hardly be said to exist."

After properly denying the existence of such a thing as *general excess of action*, the author concludes,

"45. If the diseases termed *universal*, by authors, be attentively examined, they will be found in general not to merit this title in the strict sense of the word; but to be most of them, without difficulty, referrible to a *primary* affection of some one particular organ."

Now, it is evident, that Dr. C. has so hedged and guarded his positions with qualifying clauses and conditions, as to render his general conclusion undeniable; but it is equally evident, that to obtain this apparent victory, he has arbitrarily given to the term *universal disease*, an interpretation which no author that we know, ever dreamt of, viz. its being that in which *every part* of the system is *at once* affected. (Paragraph 43) or as he afterwards, (paragraph 47,) more clearly expresses it, "in which all parts of the system are *immediately* disturbed by the agency of the exciting cause." By *universal diseases* are understood, those which consist in a greater or less degree of disturbance in that *combination of functions* which are more immediately necessary to life; and which is indifferently termed the *constitution* or *system*. Such especially are those, the exciting causes of which produce a *diffusive* impression upon the great connecting medium, as well as chief seat of life, viz. the nervous system; while other causes, though likewise operating directly on the same system, produce no such diffusive impression, and therefore occasion only *local* disorder, except some peculiar susceptibility be present at the time. For example, puncturing the skin of a healthy person with a *clean* lancet, will occasion only slight pain, and no farther inconvenience; but a scratch scarcely visible, with a lancet dipped in *variolous* matter, will, after a certain period, excite in those who have not previously had small-pox, an *universal* and sometimes extremely violent commotion of the febrile kind. But who would call small-pox, even thus induced, *a local disease?* And still more, who would say, that the disorder, which is essentially febrile in its nature, consists in an inflammation of the brain or its membranes, when we often see children pass through it without an hour's confinement, or ever displaying one symptom of delirium? Dr. C. may allege, indeed, that there is almost always some head-ach, and that pain of the head is always an attendant symptom of inflamed brain? True; but the presence of a single symptom does not infer the existence of a disease of which it forms only a part; for in that way we might infer the existence of every disorder of the brain, to which pain belongs. Besides, pain, and that too of the most exquisite kind, is often a mere nervous sensation, without one other symptom of inflammatory state; as in *tic douloureux:* and the most violent head-ach will be produced in some persons, by eating any thing rancid or acescent; yet we never imagine that inflammation of the brain in such cases, is brought on by eating fat meat, nor cured (as it often is) by a little magnesia, by a glass of warm water, or by emptying the stomach of its contents!

We cannot help thinking, then, that the author, whilst he has evinced much logical acumen, has gone into an unnecessary subtlety and refinement upon this head: and that the *common division* of disease into *topical or local*, and *universal or general*, is still a just and useful distinction, and as practicable in its application as any other; for whether the morbid state be prominently conspicuous in a few or in many organs at once, or whether it shews itself now in one organ or function, and now in another, the attempt to remove it can only be made *locally*, or by remedies which exert their *primary* operation upon *individual organs*, whence it is diffused to others that *partake* of the *general disorder*, and thus relieves parts to which we have no direct access, and corrects morbid conditions, the intimate nature of which we shall, perhaps, never understand.

In the second chapter, entitled, *Of the primary seat of fever in the body*, Dr. C. enumerates the phenomena of fever in its different stages and forms, chiefly taken from Dr. G. Fordyce; with occasional references to Huxam, Lind, and others; and in doing this, he (as might be expected) particularly marks those 'symptoms which may tend to shew that the brain is affected; such as *head-ach, giddiness, stupor, delirium, redness of the eyes*; and concludes that not only these, but "*all* the essential symptoms of fever may be referred, without difficulty, to a *topical* affection of the brain as their source, as will be seen by tracing their relation to the particular functions of this organ." (Page 48, 49.) Among other arguments for this, he alleges, that "the mental faculties are *constantly* observed to be more or less impaired in fevers, and in very many cases are totally deranged and obscured, while in great and fatal affections of other organs, the powers of the mind continue unimpaired to the last. Persons dying of hernia, or mortification of the intestines, any how produced, and many other diseases, are often found competent to direct the future administration of their affairs, within a few hours of their dissolution. But nothing of this kind, I believe, takes place in fever. In a few rare instances, indeed, the mental faculties appear to have been restore dimmediately before death, though they had suffered entire derangement throughou the previous stages of the disease. But in such cases, it is probable the *febrile action in the brain* had ceased, the patient dying rather of the debility, or other consequences induced by it, than of the disease itself." Page 57, 58.

Dr. Clutterbuck's respectability as a writer, and the urbanity he has uniformly shewn towards his predecessors on this subject, will effectually secure him against any thing like captious objection, or petulant criticism on our part; but as he alleges his doctrine to be *new* as well as *true*, we must be rigid, and sometimes minute, in our examination of its pretensions. Agreeably to this, then, we must notice, what others perhaps would overlook as trivial, viz. an occasional substitution of one term for another, though by no means synonymous in their strict meaning, or even in their ordinary acceptation. An instance of it occurs above. Dr. C. has repeatedly professed his belief, that *inflammation of the brain* is the proximate or essential cause of fever; but as it might naturally be expected, that when the disease has gone to such an extent as to prove fatal, palpable traces of this proximate cause would remain, as it is known to do in other parts where inflammation has destroyed life, (for example in the stomach, intestines, lungs, &c.) and as it is impossible to conceive, that the functions of the brain could be restored where such over-distension of vessels exists, the author, seemingly aware of the repugnance with which a position of this kind would be received by medical men, has softened down the *palpable* and *permanent* state of *inflammation* into the *unascertained* and *evanescent* one of *febrile action;* thereby virtually doing away all precise line of distinction, and leaving only a vague and general notion, that may be represented by whatever phrase best suits the writer's purpose at the time. The ideas generally attached to the term *inflammation*, however, are too fixed to admit of this; and the objections to the identity here implied, too strong to be thus got rid of. Accordingly, in a subsequent part of the INQUIRY, (p. 156, &c.) the question again recurs, and is then attempted to be explained away in a manner which will probably satisfy no one but the author himself.

When we had got some length in the perusal of this section, we began to suspect that we had entirely misunderstood what the author meant by *inflammation of the* BRAIN; and that, instead of *sensible increased action* and *fulness* in the vessels of that mass of nervous matter contained within the skull, it might be a mere *erethism,* without one single symptom of palpable inflammation; and this occurring in *any part* of the body. Now, if this be the case, we are contending about *words* instead of *ideas;* and in place of "giving to airy nothings, a local habitation and a name," (the author's motto) we are literally left, like the clown at the conjurer's shew, without either that which we really possessed at first, or that which we thought had been transferred to us; in short, that, agreeably to the Roman maxim of "*ubi Imperator, ibi Roma,*" Dr. C. has made the *brain* coextensive with the body, and may be fairly construed to say, *ubi nervi,*

nervi, ibi cerebrum. Thus in p. 61, he says, "the sensations of the patient in fever, uniformly point to the head as the chief seat of complaint. For as long as consciousness remains, and delirium is absent, pain or other uneasy feeling in the head, is universally complained of. Along with this, pain in the back and loins is usually conjoined, and is probably to be referred to the connexion of the spinal marrow with the *brain, of which it is in fact merely a continuation.*" And again, in page 74—"A disordered state of the stomach, is no certain proof of its being the primary seat of disease in any case, and still less in fever, in which so many other functions are disturbed. This frequent affection of the stomach in fever, it is, perhaps, not difficult to understand upon physiological principles. The stomach not only receives nerves from the great intercostal, for the support of its simple or organic life, but also *communicates directly* with the brain, by means of the eighth pair of nerves, or *par vagum*. From the *par vagum*, it probably derives its peculiar sensations and appetites, thus becoming, in some measure, an organ of sense, as well as of motion and secretion; and therefore, like the other senses *depending more immediately on the brain*, and obeying its different impulses."—Page 79. "The excitability and power of contraction of the blood-vessels, there can be no doubt, depend more or less directly on the influence of the brain and nerves. In living animals, the blood is retained within its proper channels, more by the contractility of vessels, than by the smallness of their diameters." Hence the author concludes (page 80) that 'the maculæ, petechiæ, vibices, and hæmorrhages, which so often accompany malignant fevers, are to be attributed to a torpid or nearly paralytic state of the extreme vessels, in consequence of which the blood stagnates in their extremities, or is poured into the cellular substance adjoining. In confirmation of this, it may be mentioned, that Lieutaud found the brain, after death, to be organically diseased, in *some* patients that laboured under *petechiæ sine febre.* The opinion is rendered further probable, from observing the effects of certain poisons, which manifestly exert their chief influence on the *brain* and its functions. In animals killed by the laurel water, and similar narcotic poisons, the red blood has been observed to pass into the *serous* vessels. And the bite of the serpent *hæmorrhous*, is said [*fabled?*] to occasion such a dissolution of the blood (or as I should prefer saying, such anatomy or paralysis of the extreme vessels) that it flows from every pore, destroying the patient by an universal hæmorrhage. At the same time, it is no timprobable that the blood itself, admitting it to be possessed of the living principle, undergoes a change in its properties,

'ties, in common with the living solids; though with the nature 'of this change we are unacquainted: it appears, however, to in'fluence its power of coagulation."

Had the author gone one step farther, and avowed his adoption of the late Mr. J. Hunter's opinion, viz. that the blood contains, diffused through it, a matter of the nature of brain, in which its *life* resides, *(see Hunter on the blood, &c.) his* meaning of the word brain, would then have been clear and fully expressed. Even as it is, however, we think the passages quoted will bear us out in the inference we have alleged, that Dr. C. virtually makes the *brain* comprehend the whole of the nervous system; and consequently, that as far as regards the *primary* and *chief seat* of *his* proximate cause of fever, it is the same with that of Hoffman and Cullen. We shall readily admit our error in this construction, however, should we hereafter find, that the treatment he recommends is such as can be explained by its operation in removing inflammation of that organ *usually* and *exclusively* termed *brain*. But we certainly do not anticipate such an event, after having been told *(Preface*, p. xi.) that his doctrine "at once explains, and is supported by, the effects of the best established modes of cure;" and knowing, as we do, that the various means successfully employed, are not only *very different*, but *opposite* in their powers; and that to this very circumstance, is owing their being respectively adapted to those *different* and *opposite states, which*, when they *occur collectively in a certain order, constitute* FEVER.

SECTION VI. enumerates the remote causes of fever, in which the author is disposed to enlarge the catalogue, by including whatever can, either directly or indirectly, produce morbid excitement in the brain. Thus, besides heat—cold—and their alternation—marsh miasmata—contagion, he adds *local irritation*, from certain articles eaten, as shell-fish, from worms, from passions of the mind, &c. and mentions the instance of "a little 'girl, who, from whirling herself round swiftly in play, became 'vertiginous, vomited and complained of pain in the head, to 'which succeeded increased heat and thirst. These symptoms 'continued twenty-four hours, and then terminated in a critical 'sweat.—A boy, in perfect health, went into the cellar of a brass'founder, where a quantity of charcoal was burning. He was 'seized at the moment of entrance with severe head-ach, and 'sickening immediately with a fever of the typhoid form, which 'continued for fourteen days."—Now, that the *whirling motion*, or the *carbonic vapour*, were, of themselves, adequate to produce the succeeding fever, we cannot admit; else we should find such examples of daily occurrence. They merely deranged the nervous

system,

system, to such a degree as to disturb the healthy order and vigour of the functions, and thereby gave sudden activity to some dormant tendency, as in the case which he quotes from the American Medical Museum, (No. I.) of a sailor, who, after exposure to contagion in the epidemic fever at Philadelphia, received a severe blow on the head from a cable, and was immediately attacked with the disease. Page 87.

That *derangement of any* kind, and not those merely which occasion *excitement* or *inflammation* of the brain, will bring on fever of one or other form, according to the existing predisposition, is proved by the author's own citations. "Fear, grief, 'and anxiety, as they denote an affection of the sensorium, so 'they are by almost all writers included amongst the occasional 'causes of fever. Van Swieten mentions a girl who, when in 'health, being terrified at the sight of a dormouse, fell im'mediately into a quartan ague, which continued for a whole 'winter, and was brought back again after it had ceased, by a re'petition of the same cause."—Can we imagine, not only that these act by inducing *inflammation of the brain,* but that this inflammation, after subsiding once, will recur again regularly every fourth day, for twelve months? Again, how are we to explain the notorious fact, that fear and sudden alarm have often *cured* intermittents? Is this too, by exciting inflammation?!!—To us these seem palpable contradictions, upon the principle which the author has adopted.—With regard to the phrase of *being in perfect health* before such exciting causes are applied, we must make due allowance, not only for the inaccuracy of patients whose previous feelings are all swallowed up and obliterated in their after suffering; but also, for an important truth, that *disposition to ague* will remain *latent* for several months: it is very common to find Irish labourers work all harvest in the fens, without having ague, either during that time, or through the succeeding winter while in London; but when the easterly winds set in, in spring, they are attacked very severely. Now, during all the interval between the application of the specific cause, viz. marsh miasma, and the actual coming on of the disorder, they may be so far free from complaint, as to be called in *good health;* but *such* good health is unquestionably a weakened state of certain functions, just short of palpable disease. That a corresponding state, though of more temporary duration, is produced by contagion, or by the accidental co-operation of other causes, is equally true; and it is *only under such dormant or latent state,* that charcoal fumes will excite typhus fever, or whirling motion occasion ephemera.

In the following passage, (page 88, 9) we think the author has fallen into more than one error.

"Inflammation on the skin may likewise be ranked among the exciting causes of fever, and probably acts upon the principle of irritation. Erysipelas is thus frequently succeeded by fever; when the disorder is said to *fly to the head*. This is a frequent cause of that variety of fever, which is vulgarly called *brain fever*, a term that might not unaptly be applied to every proper fever to distinguish it from other fevers. The secondary fever of small-pox, is probably to be explained in the same way; the quantity of inflammation on so irritable a part as the skin, which, like the other organs of sense, has a close sympathy with the brain, becomes in this case the exciting cause of the disease. It is in this way also, I apprehend, that violent external injuries, and operations in surgery, often terminate fatally; namely, by exciting fever, or a topical inflammation of the brain."

That inflammation of the skin excites general febrile action, and that too so violent as to destroy life, we have daily and melancholy proofs of in the accident of burns and scalds. But that the erysipelatous eruption is the *cause* of the fever which often does not *become striking and obvious* till after the external erythema has appeared, is as gratuitous an assumption, as that a partial and silent eruption of volcanic lava is the *cause* of that general and tremendous explosion which often succeeds; or that the pain, heat, and redness which take place in one extremity in gout or rheumatism, are the *causes* of a similar affection which soon after supervenes upon another. The fact is, that in each of the examples, the first appearances are only *local manifestations of a general tendency*, which will sometimes subside without farther disturbance, at others shew itself successively in different and distant parts, and at others again burst forth in a *general* and violent commotion.

The author's explanation of the secondary fever of small-pox is altogether *improbable*, inasmuch as it takes place when the suppurative process is ended, and consequently when the inflammation is *subsiding*: it appears to us clearly to arise from the absorption of variolous pus.

The last paragraph of this section, contains the virtual admission of what we have already suggested, that when Dr. C.'s *meaning* of the word *brain* is explained by his own illustrations, all the alleged novelty as well as precision of his doctrine vanish: he says—

"It would seem, therefore, that besides the specific causes of fever, namely, the different contagions and miasmata (the mode of acting of which we are ignorant of) irritation of various kinds, mental as well as bodily, if in sufficient degree, may become the exciting or occasional cause of fever. But irritation, in order to its affecting the system generally, *must* operate through the *medium* of the brain, as the common centre of feeling, and the organ of the sympathy or consent that obtains between different parts. The brain thus irritated, will have its function more or less disturbed from the natural and healthy state. That disturbance may be in every *possible degree*, and may *manifest itself in different functions*, depending perhaps upon predisposition, or other circumstances that we do not clearly understand."

Now, passing by the difficulty of *fear, grief, and terror*, especially the two former, *irritating* the brain, does he not beg the question that the brain *must be inflamed, because* the *irritating* agents which excite *manifest disturbance in different functions*, *must* operate *through the medium of the brain?* As well might we say, that a thick iron bar *must be made red hot* from a powerful electric charge sent through it, *because* this charge *melts* and even *scorifies* a small wire attached to its extremity and forming a part of the circuit! Nay farther, to adopt such a principle, would, in fact, be to adopt the Brunonian dogma, which the author himself had already (par. 44,) successfully refuted, viz. that the whole system must partake of the same state, and that the only difference consists in the disparity of this state, as occurring in one organ or part compared with another.

In the VIIth section the author alleges as a collateral proof of his doctrine, that fever is prevented by a want of sensibility in the brain; whence "idiots, maniacs, negroes, very old people, and likewise new-born infants, are less liable to fever than others; and that brutes never labour under proper fever!" A little examination, however, will shew, that much is assumed and little proved here; that some of the examples are directly against his principle, and others may be differently explained. It is well known, that few persons are more subject to violent gusts of passion than idiots; and the very essence of mania is a degree of excitement in the brain, approaching to that which constitutes phrenitic delirium. Agreeably to the *phrenitic theory of fever*, then, (if we may briefly so term it) idiots should be *more* liable than others to fever from *mental irritation*, and maniacs should scarcely ever be free from it. With regard to *negroes* when free, it has been questioned whether their intellects are not as acute as those of the *whites*; but even granting their general inferiority in this, they are not less liable to fever when in *cold* climates. Their comparative immunity in hot countries, where febrile contagion of the typhoid kind seldom operates, depends upon that peculiarity of structure in their skin, which keeps the perspiratory process steady and uniform under the highest temperature, and thereby preserves the skin cool and soft. The other natives of hot climates beneficially endeavour to maintain a similar state of skin, by frequent bathing and afterwards anointing with oil. That the nervous system in general becomes somewhat torpid in advanced age, will be readily admitted, and the comparative insusceptibility of certain febrific contagions, as of measles, scarlatina, mumps, &c. is a proof of it; but the circumstance of *very old people* being less liable to common fever, may as fairly be attributed to that very stock of vis vitæ which preserves them to great age, as to diminished sensibility of *brain*.

Infants again, are unquestionably less liable to be affected by typhous contagions than adults: but this seems merely one of those beneficent provisions of nature to prevent annihilation of the species; for if this agent were added to those producing the host of infantile pyrexial complaints, how few would arrive at puberty! As to brutes, they are certainly subjected to fewer febrific causes than civilized man; but they too have their febrile diseases, which are generally epidemic; this is most frequent in domesticated animals, and particularly cats; horned cattle also have their peculiar *distempers*, as man has his, and we are rather surprised, that the mere general assertion of Stahl on the negative side, should have been preferred to the positive and circumstantial evidence of Dr. Fordyce, whose authority ranks so high with Dr. C. Dr. F. tells us, that sheep and hogs when carried abroad in transports, were respectively liable to their peculiar epidemic maladies; but that the disease of the one genus did not affect the other. *(Diss. on Fever.)*

No one will now dispute, as a general position, that the causes of fever operate *primarily* on the nervous system, and not, as the *humoralists* believed, upon the circulating mass. We have therefore no remark to make upon the IVth Section (p. 116), in which this is formally stated, as especially favourable to the author's particular doctrine.

Chap. III. is entitled *Of the Nature of Febrile Action*, and contains a number of sections; in every one of which, while Dr. C. displays both industry of research, and dexterity in applying quotations and references, he has at the same time, through eagerness to establish his favourite position, called into his service a number of remarks, many of which are greatly overstrained in their application, and others, if strictly examined, will be found to disserve his cause. Some of these we shall shortly notice. In p. 105, he says, 'Like many other parts 'the brain is insensible in its healthy state, but feels acutely under 'inflammation; hence *in fever* every stroke of the arteries is '*dreadfully* felt, and the patient is in constant fear of the bursting of the vessels.' Had we never, ourselves, laboured under fever, this might perhaps have been admitted by analogy, as universally true; but our personal experience contradicts the assertion; having suffered severe fevers both from contagion, and marsh effluvia aided by heat, without any other sensation in the head, than that of continual buzzing noise and indistinctness of ideas; and we have often observed the same in others. He adds, that the 'whole head is preternaturally hot both to the patient's feeling and the thermometer; and this increase of heat in the *head* is often perceivable, though the *rest of the body* be cold. I know of no instance where the reverse of this takes place in fever; that is, where the *head* is *cold*, while the *body* is *hot*.' The assertion of an anonymous reviewer will probably not have much weight with the author; but as we are unconscious of any object in it beyond establishing truth, we here again speak from a strong recollection of what we noticed in ourselves while labouring under tropical fever, viz. that the *hands* were *burning hot*, whereas the *forehead* and *nose* were almost of a *corpse-like coldness*. If we mistake not, the same thing is noticed by some of the writers who have treated on this form of disease as occurring in the West-Indies.

Dr. Fordyce had remarked, that the thermometer under the tongue, shewed 105 degrees of heat, even when the patient felt himself very cold. (*Dissertation on Fever*, p. 9.) 'This, (says Dr. C.) shews an increased evolution of heat in the vessels of the head, one of the most unequivocal signs of topical inflammation.' But why does Dr. C. suppress the passage in Dr. Fordyce, immediately preceding that he quotes; and which shews, that all the other symptoms of fever *often* exist where this *unequivocal sign of* topical inflammation is absent? 'By the thermometer (Dr. F. says) he has *often* found the heat *less* than that which is found in the bodies of men in perfect health, although all the other appearances which constituted fever were present, such as foulness of the tongue, dryness of the skin, frequency of the pulse, delirium, and the patient *feeling himself* very hot. The ball of a small and delicate thermometer placed under the tongue for four or five minutes, while the mouth was kept shut, and the respiration carried on through the nostrils; has under these circumstances, been at 96, 95, or even 94 degrees, in some cases.' Id. p. 8. Where, then, we would ask, was the topical inflammation of the brain at this time? With respect too, to the great increase of heat (viz. seven degrees above health) being an *unequivocal sign* of topical inflammation, has Dr. C. forgotten Mr. Hunter's experiments, which shew that the heat in parts *highly inflamed* by *local irritation* was seldom more than *one or one* and a *half* degrees above that of health; while the heat of the skin in typhous fever, as observed by the late Dr. Currie, was often 10 degrees above health, though no inflammation could be said to exist upon the skin; for this scorching heat was dissipated in a few seconds, by merely dashing two or three bucket-fulls of cold water over the patient?

We shall quote only two more passages from this chapter, to prove the very strained construction to which Dr. Clutterbuck often has recourse in support of his theory.

P. 108.—'Excessive sensibility to impression in the organs of sense, is reckoned among the most unequivocal marks of inflamed brain: and it *probably* has no other origin when it occurs in *fever*. It is no *argument* against this, that the opposite state of *torpor* is sometimes observed to pervade *all* the functions in fever.

"In sultry weather, (says Dr. Chalmers, *Dis. of S. Carolina*, p. 150) toward the end of summer, nervous and putrid "fevers are apt to arise, sometimes accompanied, at the *beginning*, with somnolency, or an apoplectic state, which indicates much danger, though it may go off with the paroxysm, "and its return be prevented by the bark."

'These symptoms (Dr. Clutterbuck continues) are *unquestionably* the effect of *compression* of the brain, arising from increased impetus of blood towards this organ; and therefore afford a confirmation of the doctrine here advanced. The same thing is very remarkable in hydrocephalus, where the torpor succeeds to a previous state of great irritation.'

Now, without dwelling on the indirect and feeble inference couched under the word *probably*, we would say, that the circumstance of torpor pervading *all* the functions at the *beginning*, is *a very strong argument* against the *phrenitic* theory; and bears no analogy to that which *succeeds* high irritation in acute hydrocephalus. The former is clearly the same torpid state taking place in the *brain*, which is observable in the other parts of the nervous system, as the first step of fever; and when very considerable, has got the title of *Febris syncopalis*, and *F. carotica*, the faintness or stupor *going off*, in proportion as increased impetus *comes on*. The latter, on the contrary, is the *effect* of impaired tone from previous excessive action, and proceeds immediately from hydropic effusion into the ventricles, and consequent mechanical compression: this too, farther unhappily differs from the other, in being permanent.

P. 126—'The application of cold, which has of late been used with so much success in arresting the progress of fevers in their early stages, there is *great reason to believe*, is no less capable of cutting short many topical inflammations. In ileus, an affection that often *probably* depends upon inflammation of the intestines, the sudden affusion of cold water, or standing on a cold and damp floor, has almost instantaneously produced a solution of the disease. The same remedy has often cured suppression of urine, that *appeared* to depend on an inflamed state of the kidneys; and we have even instances of pneumonic inflammation being cured by similar means.

In this passage we have marked the qualifying words by putting them into Italics, so that the reader may see how guarded and cautious the author is in making his positions, and, these once granted, how comprehensive and sweeping his conclusions from them. But we must not allow Dr. C. to steal even a momentary advantage, by thus identifying *Ileus* with *Enteritis*, or, in other words, by confounding *spasm* with *inflammation*. By the term Ileus we are to understand—obstinate constipation of several days continuance, upon which at last severe cholic pains supervene, evidently arising from the violent but ineffectual efforts, which the muscular fibres of the intestines make to expel the accumulated and hardened fœces. This being properly understood, will at once explain the benefit obtained by dashing cold water upon the lower extremities in such a case; for, owing to the sympathy which prevails between them and the intestines, the fibres of the latter become relaxed, while the sudden contraction of vessels on the skin by the cold, determines a quantity of blood inwards, and occasions a copious secretion from the intestinal surface, whereby a free expulsion of their contents immediately ensues. In this way, likewise, it is, that diarrhœa so much oftener attends cold applied to the *feet*, than to any other part.

We earnestly hope that the author's reasoning on this, will not seduce any one rashly to employ such a remedy in *actual* intestinal inflammation; or, *where pulmonary inflammation really exists*, to imitate, on principle, the practice of Dr. Smollet on himself, or the accidental exposure of Dr. Blaine's patient to cold and moisture. Dr. Smollet whilst labouring under violent cough, attended with fever and severe stitches, plunged into the open sea at Nice, which brought on a fresh cold in his head, but removed his fever and stitches at once; and by repeating it recovered his strength and spirits, and removed his cough. Dr. C. observes, 'notwithstanding the Dr's. (Dr. S.'s) opinion of 'the stitches being *spasmodical*, and derived from *relaxation*, 'few at present, *probably*, will hesitate to consider the case as 'an instance of pulmonic inflammation cured by early recourse 'to the cold bath.'—!!

Dr. Blane in his account of the memorable West India hurricane of 1780, observes, that "that it had a visible good effect on "the diseases of the climate, fevers and fluxes. Chronic diar-"rhœas, the consequences of Dysenteries, were also cured by it. "But the diseases in which it operated most visibly were pul-"monic complaints. Some recent cases of phthisis, and even "the acute state of pleurisy were cured by it. Nay, in the more "advanced and incurable state of phthisis, the hectic fever was "in a great measure removed, and a temporary alleviation at "least procured."---'A lady of his acquaintance who was ill of 'pleurisy at the time of the hurricane, and passed more than 'ten hours in the open air, sitting generally in a plash of water 'from the rain that fell: she afterwards felt no more of her 'complaint, nor had any return of it.'

Would Dr. C. really have us believe, that *fevers*, *fluxes*, *chronic diarrhœas*, *recent phthisis*, and *pleurisy*, diseases so different in their nature, were all alleviated or cured by exposure to cold and wet? !!! If so, *most probably*, he will hereafter give us cases of each, where these means were successfully employed under his own direction. But it may be necessary to apprize our junior readers, that Dr. Blane never suggested any such opinion, and consequently, that both the novelty and *merit* of the idea, belong exclusively to Dr. C.---Dr. Blane expressly considers the benefit in all these cases, to have proceeded from some great amelioration in the *quality* of the atmosphere, independantly of temperature: and that this is the true explanation, we have a striking collateral proof in the instance of the Harmattan, a *hot* and *extremely parching* wind which blows at certain seasons on the Guinea coast. As soon as this sets in, all fevers, fluxes, and epidemic complaints previously prevailing, are either checked or entirely suspended. Even small pox, though extremely virulent at the time, is rendered mild by it, and its farther dissemination put a stop to. See Mr. Norris's account of it in the Edin. Med. Com.

Our extracts and remarks already extend to such a length, as to demand that we should go but little farther at present; more especially as we shall soon have occasion to return to the subject in our notice of Dr. Beddoes's treatise. We shall, therefore, at once, proceed to the IX Sect. p. 156, entitled, *Of Dissection as illustrating the Nature of Fever*, which we think will prove the *crux* of the whole doctrine. Dr. C. indeed seems to anticipate this as likely to be the prevailing sentiment of his readers; and, accordingly, like a cautious General, his first endeavours are, to take such steps as may lessen the disgrace of failure, provided he should not succeed in this attempt to establish his chief positions. He says,

'The arguments hitherto advanced in support of the opinion, that fever consists essentially in a topical inflammation of the brain, are derived principally from analogy, and an investigation of the phenomena of the disease in relation to the peculiar functions of that organ. It might naturally be expected, that dissection of the bodies of those in whom the disease had proved fatal, would remove all doubt from the subject; and at once either satisfactorily establish or overthrow the opinion in question.'

'But although much light is undoubtedly to be derived from this source, and we shall find, in fact, that every support is afforded to the supposition that could reasonably have been looked for, yet the evidence furnished by dissection is not conclusive; and that for different reasons.'

'In the first place, the wonderfully minute and delicate structure of the brain renders it very unfavourable for accurate examination*. Of this complicated organ, made up of numerous parts, the particular uses of which are almost wholly unknown, our knowledge is exceedingly limited. It more quickly undergoes a change towards putrefaction than almost any other organ of the body, and previously to putrefaction becomes soft and unresisting, rendering difficult or fruitless all attempts to observe it narrowly. This tendency to decomposition in the brain comes on, in general, so rapidly after death, that we are *probably* but little acquainted with its perfectly sound and natural appearance.

'It is next to be considered, that by far the greater number of demonstrations of the human body, given by anatomists in the schools, and from which our knowledge of the structure and appearance of the brain is principally derived, are of subjects destroyed by diseases, many of them, no doubt, of this very organ. And when we reflect farther on the sources from which the anatomical theatres are chiefly supplied, viz. the most indigent classes of society, a large proportion of whom is daily cut off by fevers, it must appear highly *probable*, that what is considered and exhibited as the natural and healthy state of parts, is in reality a diseased one, *perhaps* the immediate consequence of fever itself, and which we have not yet clearly learned to distinguish from the state of health. It is almost needless to observe, with respect to this, that where the sound and natural state of a part is imperfectly known, it is quite impossible that the morbid changes of structure to which it is liable should be well understood. What we, therefore, so commonly meet with in books, as to the state of the brain in regard to hardness, softness, colour, fulness, or emptiness of vessels, &c. is *perhaps* little to be relied upon. Not that such appearances do not really exist, or are unfaithfully described, but because we have no certain standard of health, to which we can refer them as objects of comparison. Nor can we readily tell, in all cases, whether the changes observed are the effects of disease, or of a beginning decomposition.'

In addition to these reasons, he states the incompetence of these who have taken upon them to observe and report on the state of this organ after death; the bias of preconceived theories; and the dread of infection from dead bodies; and concludes,

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* 'The substance of the brain is so soft, and the fibres so tender, that they can hardly bear to be touched without breaking:—anatomy has not yet arrived at that degree of perfection, as to make a true dissection of the brain. (Steno on the brain: see Winslow, sect. 10, p. 195.)'

'From one or other of the causes mentioned, examinations of the brain after fever have been very rarely undertaken. I am inclined to doubt indeed, from the inquiries I have made, whether half a dozen such dissections have *purposely* been instituted in this metropolis within as many years past: while, in all probability a great number of brains of patients dying of fever have been dissected within the same period, and more than this, have been exhibited to the students as the natural condition of the organ.'

The foregoing quotations abundantly evince the author's determination, to make his positions good by *hook or by crook*; but whether *assertion* will be admitted as *proof*, whether he will be able to persuade others, that inflammation is present when no inflammation can be *seen*, must be left to time to decide. It strongly reminds us of Swift's admirable irony upon the doctrine of transubstantiation: and the poor anatomical teachers, who, are said to exhibit scarcely any but *diseased* brains, and consequently not to know what the *healthy* state is, appear to stand exactly in the same predicament with regard to Dr. C. that brother John and brother Martin did with their elder brother Peter, who were held to be either very stupid or very obstinate, because they would not say that that was present in the bread, which *their senses* could not in any way detect, and which their *reason* told them it was absurd to suppose without palpable proof. When the parties thus charged with habitual error, and incompetence to judge, are so numerous, and so well able to defend themselves, *we* need not enter the lists on their account; but for our own part, we would merely say, that *we* have never observed that rapid putrefaction in the brain which Dr. C. alleges; on the contrary, that when not previously exposed by accident or operation, it has appeared *slower* in becoming putrid than almost any other organ of the body. As to the quotation from Winslow and Steno (*see note in* p. 216,) it plainly refers to the unsuccessful attempt at tracing the course of those fibres which have been said to exist in, and compose the brain, with a view to ascertain its intimate structure, and explain its peculiar functions; and by no means implies, that *signs of inflammation*, or other organic derangement, are not as distinctly cognizable in the brain as in other parts.

The final argument adduced by the author on this point, rests upon the obvious fact, that the vessels of the medullary portion of the brain, are so minute as to be altogether invisible in the sound state; whence, he says, it is not at all surprising that they should not, even under inflammation have their capacities sufficiently enlarged as to admit the colouring parts of the blood. (p. 165.)

(p. 165.) 'Redness (then, in his estimation) is certainly not 'an essential character of inflammation (!!) In hydrocephalus internus, which, it is now universally (?) agreed, 'owes its origin to inflammation, the only change, oftentimes, that can be observed in the parts after death, is a thickening and opacity of the *tunica arachnoidea*, a membrane 'which in the sound state is pellucid, and of extreme tenuity.

'There is *no* difficulty then, in supposing, that the brain may have undergone material changes in its structure from inflammation, though our senses are incompetent to detect them.' (!!!) '*Admitting* therefore, that the *medullary* substance of the brain is the primary seat of morbid affection in fever, a supposition that is warranted by the lesion of its functions, so constantly observed throughout every stage of fever—we are not, for the reasons mentioned, to look for *visible change* of structure in every instance of fatal termination.' (!!!)

On these two paragraphs, (several words of which we have put in Italics,) we would briefly remark, that every author who has treated the subject of inflammation, has admitted *redness* to be an *essential* symptom; in other words, that inflammation consists in augmented action, and *increased quantity of blood*, in the smallest *red* vessels. Boerhaave, indeed, and Van Swieten, talk of *lymphatic* and *serous inflammation* in colourless arteries; but to believe in this, we must recal the exploded ideas of Loewenhoeck respecting a decreasing series of particles in the blood, and of vessels respectively fitted to convey these. In the instance of *hydrocephalus internus*, the author has evidently confounded *two* forms of disease, widely differing from each other, viz. the *chronic* hydrocephalus, which depends on mere weakness of the exhalant arteries, without any inflammatory state of the red branches; and the *acute*, in which more or less appearance of inflammation is always found upon the *surface* of the brain, but none, or scarcely any, in the ventricles where the water is effused. It is not unworthy of notice too, that Dr. C. assumes the *medullary* portion of the brain, which has scarcely any *red* vessels, to be the seat of fever; while the *cineritious* part which is abundantly supplied with them, and in which, consequently, increased redness, as indicating an inflammatory state, would have been readily detected, is left out of the question.

The remainder of the volume is chiefly occupied with a general account of the several remedies commonly employed in fever; as *blood-letting, vomiting, purging, sudorifics, epispastics, relaxants, and antispasmodics, simple stimulants, cinchona, sensorial stimuli, cold, and mercury*; on each of which remarks are made, and each recommended in its turn.

The reader has certainly now before him, sufficient evidence to guide his decision, and enough, we trust, to bear *us* out in the opinion we are about to give; which is, that we think the author has completely failed in establishing his leading principle, viz. that *the proximate cause of fever consists in topical inflammation of the brain.* In saying this, however, let it not be supposed that we mean either censure or disparagement to Dr. C. for we are equally ready to declare, that he has, throughout his work, afforded unequivocal proofs of respectable talent—of great industry—and of competent learning in his profession. We only regret that these should have been expended upon such a visionary undertaking, when so many substantial ones invited his labours; and we recommend his Inquiry to the attention of our readers, fully satisfied, that although, like ourselves, they will probably not be made converts to his *doctrine,* they will yet receive both amusement and instruction from a perusal of his book.

Art. V. *Observations on the Inflammatory Affections of the Mucous Membrane of the Bronchiæ.* By Charles Badham, M. D. Member of Pembroke College, Oxford; Physician to His Royal Highness the Duke of Sussex, and to his Household; Senior Physician to the Westminster General Dispensary; and Lecturer on the Practice of Physic and Chemistry. 12mo. pp. 133. 4s. London, Callow.

THE author of the work before us affirms that the inflammatory diseases of the lungs are imperfectly known; and his object therefore, is to add to our knowledge, chiefly by presenting more clear views of the general subject, but in part also by describing an affection which has never hitherto been precisely distinguished.

The lungs are unquestionably subject to a greater variety of severe disorders than any other viscus in the body. Yet, when we number those acute affections with which we are already familiar, pleurisy, peripneumony, consumption, croup, and catarrh, we shall not readily divine where another inflammatory disease can be found. "The disease," says Dr. Badham, "which forms the subject of the following pages, is an inflammatory affection of *that part* of the mucous membrane which lines the bronchial tubes, and perhaps also of the vesiculæ in which they terminate." "The inflammation of the bronchial membrane, in its most acute form, though it has been touched upon in various writings on practical medicine, has never been regularly described, and has obtained no distinctive appellation, and therefore no place in systems of nosology."

Now, when we call to mind the structure of what is styled the substance of the lungs; when we remember that it consists of two thin membranes, joined by a delicate interlacement of cellular texture through which the vessels ramify; that the whole is in fact little more than a convoluted surface, capable of containing between its folds upwards of 200 cubic inches of air*; we cannot without some difficulty yield to these additionally minute distinctions between within and without. But thin as this substance is, we have already been made acquainted with inflammations of the outside, the middle, and the inside; and therefore, unless our author can mark still more precisely, the limits within which he stands, we must be bold enough to deny that he is occupying neutral ground.

In two preliminary chapters indeed, he details the opinions, many of them not very relevant to his subject, of several ancient and modern physicians respecting bronchial inflammation. He quotes, too, some doctrines from his cotemporaries respecting peripneumonia notha, and some dissections by Mr. Chevalier and other friends, in which the bronchiæ were inflamed; but concerning any thing further which his cotemporaries may have said respecting inflammation affecting the lining membrane of the lungs, he is totally silent. We hope that his silence has arisen only from his ignorance. Far be it from us to blame him for attempting to catch a portion of the "spirit breathed from dead men to their kind"—yet, would it not have profited him more, to have spared a little of the time spent in poring over accounts of defluxions and infarctions, to have seen rather more accurately what was passing in the living world before him? We could quote at least four of his cotemporaries, who have spoken in language by no means ambiguous, of acute inflammations of the lining membrane of the lungs; yet of those, not one word is said by Dr. Badham in his learned preliminary chapters.

Our author, in speaking of catarrh, talks of the "limited affection of the nostrils and the sinuses of the head, to which the term is *now* appropriated." Yet Dr. Bree, in treating of the same subject, mentions "inflammatory affections of the mucous glands which line the passage of the nose, fauces, and trachea, as far as its divisions, and possibly *lower in the breast*, but not extending to the extremities of the air pipes*."

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* See Goodwin and others.

Dr. Baillie observes, "when a portion of the lungs is inflamed, its spongy structure appears much redder than usual; the colour being chiefly florid, but partly of a darker hue. This arises from a much greater *number of small vessels than usual, being distributed upon the cells of the lung, which are capable of admitting the red globules of the blood.*"

So rarely do inflammations in this part seem to affect the membranes separately, that Dr. Baillie remarks in the same page in which he describes the inflamed substance and air-cells, "Inflammation of the substance of the lungs, I believe, seldom takes place without some similar affection of the pleura; at least, in the instances which I have seen, this has been most frequently the case†." The same clear and intelligent author states, that "when the inner membrane of the trachea is inflamed, it is sometimes lined with a layer of yellowish pulpy matter. It extends from the upper part of the cavity of the larynx into the *small branches of the trachea, which are distributed through the substance of the lungs*; there is at the same time, a good deal of mucus in the trachea and its branches, together with a *mixture of pus*. This is the appearance of the inside of the trachea in persons who have died from the croup‡."

Darwin, under the head of peripneumonia trachealis, says, "the croup is an inflammation of the upper part, and the peripneumonia of the lower part of the same organ, viz. the trachea or windpipe‖." Dr. Badham, by a singular coincidence, when speaking of his undescribed and unnamed bronchitis acuta, asserts, though untruly, as appears by the foregoing quotation from Baillie, "that there is a striking analogy between this disease and the cynanche trachealis; and that it is in fact, an extension of the inflammation, which in the croup is confined to the trachea." In another place, Darwin maintains that "chincough is an inflammation of the membrane which lines the air vessels of the lungs¶." Still more distinctly he affirms, that "the superficial or spurious peripneumony consists in an inflammation of the membrane which lines the bronchiæ, and bears the same analogy to the true peripneumony, as the inflammations of other membranes do to that of their parenchyma, or substantial parts of the viscus which they surround*." The last extract which we shall give on this subject is from Dr. Reid; "A cold, if the term be applied to denote inflammatory defluxion from that portion of the vascular, and very irritable membrane, which is not merely confined to the nostrils, fauces, and trachea, but is extended through the whole internal surface of the lungs, has not merely an intimate connexion with an actual inflammation of these organs, but is in reality the same disease, differing alone in degree. This identity of catarrhal and pulmonary inflammation it is important to recollect. Nothing, perhaps, is more injurious to medical science, than an exuberant multiplicity of nosological distinctions, which, in reality serve no other purpose than to mislead and betray. By regarding that membrane, which lines the internal surface of the nostrils, is extended through the bronchiæ, and at length forms the numerous air vesicles of the lungs, as one continued very vascular and irritable covering to this large expansion of surface, comprehension will be materially assisted in tracing the intimate connexion of the respective disorders of these organs, that have been erroneously supposed to possess a specific variety of character†."

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* Practical Inquiry into disordered Respiration. 4th ed. p. 108.

† Morbid Anatomy. Page 63.

‡ Ibidem. Page 91.

‖ Zoonomia. Vol. 3. p. 316.

¶ Ibid. Page 376.

To this mass of extracts Dr. Badham may possibly reply—True it is, that these inflammations were sufficiently well known to exist—but was it known how to distinguish and treat them? We answer, that it was at least as well known in 1807, as it is in 1808. He has not on these subjects said one word of any importance. He says—and he says truly—that these affections are marked by an infinite variety of symptoms, according to the circumstances and constitution of the patient; and he leaves the treatment—where he found it—to be regulated by a just or unjust measure of these circumstances in each individual instance. In treating of the acute form, which he told us had never been regularly described, he shows that it will often happen that a strong man will be seized, after severe exertion or exposure, with great pain in the breast, difficulty of breathing, heat, thirst, and all the usual constitutional symptoms of inflammation. By what means can he or any man detect with any approach to certainty, the precise membrane, or as Dr. B. expresses it, "part of a membrane" which is diseased? Or to what possible use could he apply his information, if he were lucky enough to obtain it? Would it enable him to supply his apothecary with any better means of relief than the lancet? Or would it guide him in the use of it by any tests more certain than the appearance of the blood, the state of respiration, and the state of the pulse? We hold that, in such cases, it is not only impossible to say where the precise seat of inflammation is, but that it would not be useful if it were possible; for we believe with Baillie, and, as we suppose, with almost every man of practice, that where such severe inflammations exist, if they do not soon cease by death or recovery, they will not long be confined to one part of a membrane, but will readily extend from the surface of the air-cells to the surface of the pleura.

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* Zoonomia. Vol. 3. Page 375.

† Treatise on Consumption. Page 105.

From the analogy of croup, in which Dr. Badham supposes that calomel is, perhaps, our best remedy, he believes the same medicine likely to be highly beneficial in this affection. He has never yet fairly tried it in any acute form of the disease, but professes his intention to do so, when opportunity occurs, by giving it, as in croup, in small and repeated doses. Calomel is certainly a very powerful medicine, in certain classes of inflammation. All of those, however, in which it is known to be decidedly useful are marked by some departure from the laws of simple inflammatory action. It is the opinion of Baillie that even in croup, in which its utility is more uncertain, "the whitish membrane is formed by some peculiar action of the blood-vessels of the inner surface of the larynx and the trachea, which is superadded to inflammation." The use of calomel, therefore, in the more simply inflammatory state of the lungs, we think likely to be still more doubtful than in croup. We trust that it will be very long before a full trial of its powers, in these severe affections is made, for such a trial cannot be consistent with copious bleeding.

Our author speaks of digitalis in a manner calculated to do much harm. It is customary, he says, to give this drug in doses which are far too small. "It is rarely given in greater quantity than 10 drops of the tincture for a dose, and in this quantity it rarely succeeds in any disease. But the fact is, that children of two or three years old will often take *much greater doses* without inconvenience." Most of our readers must have witnessed the mischief arising from the accumulated effects of this medicine. We have known two instances in which large doses given to adults, produced extreme disorder of the head and of the stomach, attended with convulsions. When we further add to this experience, a consideration of our uncertainty of the beneficial effects of digitalis in acutely inflammatory complaints, and of our ignorance of its mode of operation,

tion, we cannot easily be persuaded to follow our author's dangerous advice, especially as it relates to children.

Having no further observations to offer on the author's account of this hitherto irregularly described disease, we shall now proceed to inquire how he has executed the other part of the task, which he has imposed upon himself. Whether his readers are likely to obtain by his means, clearer ideas concerning inflammations of the lungs, may perhaps, be shortly determined by a perusal of the following extract. "The disease to which we find the bronchiæ actually subject, is commonly inflammatory; but there is reason to believe, that the ordinary view of this affection does not extend beyond a common catarrh. However we may be assured that this is not all. The affection of the bronchiæ from cold may be catarrhal or not catarrhal, and yet inflammatory in either case. In its severer forms it has often no analogy whatever to catarrh, for the inflammations of the mucous membrane are chiefly of the asthenic character, the catarrhal remarkably so; but the most striking disease of the genus does not belong to the asthenic inflammations; and there is, besides, so great a variety in the degree, as well as in the essential character of these affections, and they are liable to so many modifications from season, age, and other circumstances, that the ambiguous language in which they have been described, affords no occasion for surprise." Those who may have been appalled by the view of this hopeless confusion, will be in no small degree relieved when they find that these hitherto refractory varieties peaceably subside at the approach of Dr. Badham, into three; and that these three, in fact, are no other than their old familiar acquaintances, acute inflammation, peripneumonia notha, and catarrhus senilis. This happy knowledge however, is not to be communicated at once, but is to break on them by slow degrees through the new denominations of *bronchitis acuta, bronchitis asthenica,* and *bronchitis chronica.*

Dr. Badham's doctrine then, is simply this: that the inner membrane of the lungs is subject to three species of inflammation—the acute, the chronic, and the intermediate. The received opinions on this subject are, that the lungs are liable to severe inflammations which often affect their inner membrane as well as the pleura; that they are also attacked by an obscure minor inflammatory complaint; and that in chronic catarrhs they are loaded with mucus, discharged by the glands and vessels of the bronchiæ. In what the superior accuracy of Dr. Badham's views consists we are at a loss to discover. How he became entitled to assume that peripneumonia notha is nothing more than a sub-inflammatory state of the bronchial membrane, we cannot tell. The affection is very obscure, and he has by no means made it less so. Since he has added nothing either to the history or the treatment of the disease, we shall not plague our readers or ourselves with any account of it. What Dr. Badham may choose to guess about it can be of no consequence to any one but himself. Why chronic catarrh should be called an inflammation, and styled bronchitis we cannot discern; unless it be that it may fall quietly into a line with the rest, and not disturb the author's nosological uniformity.

Remembering, then, what our author asserted respecting the first of these affections having received no distinctive appellation, and therefore no place in systems of nosology, we find that his lucid labours resolve themselves into the invention of nosological epithets. Such an achievement may, indeed, be strictly commensurate with his powers, but their exertion was not called for on the present occasion. His bronchitis acuta, as we have shewn, was already known in fact, and was justly considered as a frequent attendant on catarrh, peripneumony, croup, hooping cough, and measles: but if still something were deficient; if it were absolutely necessary that it should be denoted by a specific name as a separate disease, that too had been done by Darwin, under the title of peripneumonia superficialis.

Change, when it is not useful, must always tend to perplex. The arrangements of nosology, where they *follow* the advances of medicine, are, we think, highly useful as aids to the memory; as simple and compendious methods of recalling and expressing our knowledge. When the nature of any obscure disease is rendered more obvious, this new knowledge, if stamped with an appropriate name, will pass more currently through the world, and be more quickly appreciated than it otherwise might be. Every system of nosology, however, has outrun the progress of medicine, and hence has, in numerous instances, rather rivetted error than perpetuated truth. Whether the labours of Dr. Badham be of this latter description, we really consider it very immaterial to inquire; we think, however, that they are so. He lays down, as by the sweep of his compasses, the breadth and thickness of his diseases in a manner which nature despises, and which, if it have any effect upon his readers, will have a bad one.

The language of this writer is often absurdly inflated. He talks of "mutations of weather," and still more finely "commutations of air." If he go one little step further, half his readers will not know what he means when he speaks of a change in the weather. "Let me not understand thee then, speak it in Greek."

His work has, after all, one merit of no common kind, and one which we anxiously wish that we had more frequent opportunities to praise. His book is a duodecimo of 133 pages, which may be bought for four shillings.

We now leave our author in full possession of all the pleasure and all the pride which he can derive from our unequivocal avowal, that he has added three items to the long list of hard words, of which we had already too many. He, and he alone, has talked of *bronchitis*. By the rule "*ex pede Herculem*," we should not judge Dr. Badham to be a giant. We remember an officer, who on foreign service was chiefly remarkable for his care to preserve and burnish his epaulettes, and of whom it was aptly said in consequence, that he would never be a Buonaparte. Judging from similar evidence, we may venture to predict that Dr. Badham, though he was educated at Pembroke College, is Physician to a Royal Highness, and has a long train of et cæteras, will never be a Harvey.

Art. VI. *Observations on Lithotomy; being a Republication of Dr. James Douglas's Appendix to his History of the Lateral Operation for the Stone, and of the other Original Papers relative to Mr. Cheselden's Invention and Improvement of that Operation. To which is added a Proposal for a New Manner of Cutting for the Stone.* By John Thomson, M.D. one of the Surgeons of the Royal Infirmary, &c. &c. 8vo. pp. 91. 2s. 6d. Edinburgh, Blackwood. London, Longman and Co.

PROFESSOR Thomson, conceiving the operation of Mr. Cheselden still to remain unrivalled in the essential points of facility and safety, and finding it still but very imperfectly understood and partially represented, has republished in an 8vo. pamphlet the progressive improvements which the lateral method received in the hands of that celebrated surgeon. The first and third of his operations were described at length by the ingenious Dr. Douglas, in his Postscript and Appendix to the History of the Lateral Operation, published at London in 1726 and 1731. The second was very briefly sketched by Mr. Cheselden himself, in an Appendix to the fourth edition of his Anatomy. To these valuable documents, the editor with a very creditable anxiety for the interests of the profession, has added M. Le Dran's improved method in the words of his translator, with the view of enabling the reader to compare it with a proposal which it in some respects resembles, for a new manner of cutting, *by himself.* This however, he does not venture to propose as an improvement on Mr. Cheselden's operation, but as less liable to objection than any of the cutting gorgets now in use. As the novelty of this proposal consists not in the description of parts to be divided, but simply in the form and use of the instrument with which their division is made, we shall, previously to the consideration of its merits, present our readers with an abstract of the various instruments that have been employed or proposed by operators of repute for the performance of the lateral operation.

The number of operations proposed, and the variety of instruments employed, have rendered the history of lithotomy formidably voluminous. Under these heads the subject might be advantageously discussed, but it would be altogether foreign to our present purpose to treat specifically of the former, since the operations under review differ only in the mode of performance, and fortunately for mankind, surgeons are now unanimous in their adoption of the lateral method. The present itch for invention, which the sequel will go far to prove contagious, extends only to the figure of a knife. A more correct anatomy has exposed the barbarism of Marianus and his disciples, and the latest, although unquestionably the greatest improvement of the hypogastric section by M. Deschamps, affords no prospect of the revival of its popularity. For the most easy and successful method of extracting the stone from the bladder, we are indebted to the unskilled audacity of an itinerant monk*, from whom the pitiful pride of the French surgeons disdained to derive that information, which established the fame of Raw in Holland, and Cheselden in England. Of Raw's success we think no reasonable doubt can be entertained†, but his principles were as truly empirical as those of his ignoble prototype. The very unphilosophical reserve of the Leyden professor has left his practice as well as his character open to controversy. Douglas, Morand, Garangeot, Le Cat, Bertrandi, and the most famous of our countrymen either doubt or deny the practicability of the operation ascribed to him by Albinus. But of its impossibility the clearest demonstration is given by Camper‡. It is to the penetrating genius and unwearied activity of Mr. Cheselden that we owe the establishment upon scientific principles of an operation, the success of which as practised by himself and his followers, has afforded the most exemplary proof of their skill.

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* This is however denied by some respectable authorities. Vide Bertrandi, Deschamps, and others.

† Though of this the learned Professor Camper insinuates some suspicion.

‡ Vid. Demonst. Anat. Pathol. Vol. II. Pl. III. Fig. 4.

The operation of entering the bladder per perineum, properly consists of an external and an internal incision, although antecedently to the invention of the cutting gorget and the concealed bistoury, the two incisions were more or less blended, and performed by the same knife. The best form of instrument for making the second or internal incision, which includes the prostate gland and neck of the bladder, is precisely the question now *sub judice*.

Franco, to whom M. Deschamps gives the invention of the lateral method*, used, as he informs us, a double-edged razor, with which he cut the parts about the neck of the bladder. Jacques occasionally employed his own razor; his knife was somewhat longer and slenderer than that commonly used in his time. A large spear-pointed bistoury resembling a lancet, and clothed within an inch of the point, was the lithotome in general use, and that which according to Heister was used by Raw, though Albinus describes it differently. Mr. Cheselden's was a gently convex blade ending in a sharp middle point, formed on the opposite side by the sloping of the beak for half an inch. The bistoury of Le Dran was an abscess lancet, encased in a sliding shaft. That of Garangeot was only smaller than Cheselden's, having a blunt shoulder of greater length than the blade. Le Dran's was the model of that used by the French, and the form of Cheselden's is preserved among us with but trifling alterations to the present day. Similar varieties prevailed in the form of the instrument for directing the forceps. Raw and Senffius of Berlin employed the male and female, or ensiform conductors; the single canulated conductor of Franco and Hildanus was recommended by Mery, and preferred by Mareschall and Le Dran, and from this was evidently taken the idea of Cheselden's and the blunt gorget used *at this* day. The precise form of James's conductor it is not now possible to ascertain with accuracy. By Bertrandi it is likened to a myrtle leaf, by Garangeot to an eraser, by Bussiere to a lozenge, and by Heister to a spoon. It is generally represented as a simple dilatatoire, but Garangeot has the following note upon his extract from Mery†. "Je n'ai point fait graver cet instrument parce qu'il est *mauvais*. C'est un petit instrument *tranchant*, ressemblant a une gratoire pour

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* Dr. Douglas denies that he did more than extend the section of the bulb to the apex of the prostate. Vide Append. p. 53.

† Vide Traite des Operations de Chirurg. Tom. ii. P. 134.

ratisser les ratures du papier; a la difference qu'il n'est *tranchant que d'un côté*, et que son manche est une longue queüe d'acier*."

A very principal improvement of this operation was contributed by M. Mery, viz. the use of a grooved staff. He also recommended a curved bistoury, fixed at the handle, its edge convex, terminating in a stilet of an inch long, and pointed to play in the groove of his staff. It was a circumstance unfortunate for his posthumous reputation, that he abandoned the operation without making trial of its merits. Mr. Cheselden in his first operation, after injecting the bladder and cutting freely through the skin and fat by the side of the sphincter ani, thrust a crooked knife, cutting on its concave edge, between the vesicula seminalis and ischium of the same side into the bladder; then lowering his hand so as to continue the incision, till the point of his knife appeared at the upper extremity of the wound. Here he derived no further aid from his staff, than the depression and steadiness of the bladder. This was undertaken in imitation of the method of Raw, as described by Albinus†. In his second and third operations, Mr. Cheselden used only the scalpel before described, with which he cut in a straight line upwards or from himself in the one, and downwards or towards himself in the other.

Garangeot proposed in the second edition of his Operations de Chirurg. 1731, with the small knife before described to enter the bladder, by turning the wrist and bistoury in the coup de maitre or deep incision, so that the cutting edge which was in the first incision turned towards the fingers, should face in this the back of the hand. This is an evolution which we confess we never did quite comprehend, and the plates of Garangeot exhibit little else but the huge figures and costume of chirurgiens and aide chirurgiens.

M. Foubert, [Mem. de l'Acad. de Chirurgie, Tom. I.] finding the operation of Raw, which was said to consist in opening the body and not the neck of the bladder, incapable of performance in the manner represented, that is by cutting upon the staff, invented an apparatus comprising a long and straight trocar and canula, a lithotome of which the blade long and tapering to a point formed an acute angle with the handle; and a gorgeret or dilatatoire of two concave branches, which opened by an approximation of the handles. Introducing the fore-finger of his left hand into the rectum, he depressed it towards the base of the right thigh, and plunged his trocar and canula into the bladder at the distance of four or five lines above the orifice of the urethra, and at the same distance laterally from the neck of the bladder. Then taking the handle of the trocar in his left hand, he slid the blade of his lithotome upon a groove in the back of that instrument with his right, and by first depressing the handle to raise the point, and as he withdrew it, depressing the point to raise the handle, he made a free wound in an inclined direction towards the raphe. This contrary manœuvre finished the incision. Having retired the lithotome, the gorgeret was introduced by favour of the grooved trocar as the knife had been before. In place of injecting the bladder, an unnatural and painful practice, M. Foubert suffered the patient to retain his urine, which he suppressed artificially. Five successive cases presented a result highly satisfactory, but it was soon discovered that unless the bladder was well distended, the trocar never entered its cavity, while from the collapse of this viscus it of course happened that the urine had no free exit; which, together with the structure of the parts, predisposed considerably to the formation of fistulas and sinuses. To prevent this consequence, a canula was retained in the wound. Under this improvement, M. Foubert lost one of nine patients cut at the charity hospital in Paris.

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* If this account is to be relied on, it is evident that the cutting gorget was the original instrument of Jacques. This however, detracts nothing from the merit of Hawkins.

† Index Supellectilis Anatom. &c. Leid. 1725.

In 1748, a monk named St. Cosmes, published in the "Journal des Savans," an account of a new instrument for the excision of the stone. This was the lithotome caché, the most ingenious contrivance that the history of the operation presents, and one that not undeservedly attained a reputation which it has in some measure preserved to this day. A particular description of the instrument may be seen in the 3d. vol. of the Acad. de Chirurg. par M. Louis. It consists of a bistoury blade lodged in a hollow style or probe, fixed to a wooden handle, and capable of being projected to a greater or less extent at the pleasure of the operator. It is used in the act of its removal. Notwithstanding the high authority of its supporters, (for the testimonies of Boudon, Tardy, Hevin, Falconet and others were unequivocally given in its favour,) and the numerous instances adduced of its practical superiority, it was most furiously deprecated by M. Le Cat, and became the subject of a long periodical warfare between him and its projector. M. Cacqué, to meet one of the principal objections of Le Cat and Louis, viz. the danger lest the point of the instrument should pierce the side of the bladder, proposed to leave the point and edge of the blade blunt, to the extent of four or five lines;

and M. Thomas of the hospital de Bicetre contrived a very complicated instrument, compounded of the trocar of Foubert and the bistouri of Frere Cosmes. The trocar was spear pointed, nearly resembling that of the bistoire caché; the blade of a knife was concealed by a similar construction in the canula of the trocar, and to the back of this canula a small conductor was adapted. Having punctured the bladder a finger's breadth below the arch of the pubes, the bistoury was turned a little obliquely towards the tuber ischii, and thrown out to the desired extent, as it was withdrawn in that direction. At the same time the conductor was retained in situ by the left hand of the operator, to serve for the entry of the forceps. M. Thomas could regulate the extent of the wound by raising or depressing the blade. The stated object of these several alterations was to obtain a greater certainty of entering the bladder, to render the extent and direction of the incision more definite, and thereby less endanger the intestine, vesiculæ and pudic artery.

Nicholas Le Cat, "*vir acris, minimeque sibi diffidens, aut malignus proprioruim meritorum estimator**," described two Scalpels of a peculiar figure†. The first was flat, shaped like a leaf, cutting on both edges and grooved in the middle; with this the incision of the integuments and membranous portion of the Urethra was performed, and when the point was lodged in the groove of the staff, it was held there by an assistant, (for the operator's left hand was occupied by the staff,) while a long knife with a convex edge ending in a reflected point, precisely resembling an abscess lancet, was run in upon the groove of the first, and carried obliquely inward and downward to divide the prostate and neck.

After exposing with a somewhat invidious assiduity the defects of the concealed bistoury, the same M. Le Cat with an excellent assurance proposed an instrument, without exception the most operose and unscientific ever used in Lithotomy‡. It was another attempt to revive cutting the fundus, after the failures of Raw, Bamber, Cheselden, and Foubert. Having fairly demonstrated that this could not be effected by cutting upon the staff from without, he contrived a catheter slightly curved and split from the curvature to the point; having its posterior or convex section moveable upon the anterior or concave by means of a wire stilet attached to it, and contained in the hollow of the instrument. By thrusting down the wire he projected the moveable leg, so as to make it the base of a triangle, of which the fixed end of the staff and wire formed the sides; the anterior angle protruded when the catheter was drawn up a finger's breadth below the ostium vesicæ, but he cut upon it with his uretotome before mentioned, between and above the ureters or by the side and above the left vesicula seminalis. His three experiments 'sur les Corps vivans,' are the best commentary upon the operation. The first, we think, lived seventeen days; the second, three months; and the third four days!!

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* Haller's Biblioth. Chirurg. Vol 2.

† Histoire de l'Acad. 1738.

‡ Recueil des pieces concernant l'operation de la Taille, & additions au recueil 1753, A. Rouen.

In 1749, Mr. Mudge, a surgeon of Plymouth, proposed to remedy the only remaining defect of the lateral operation, which he conceived to be the difficulty of removing large calculi, by a dilating knife to ride on the left cheek of the forceps*.

M. Le Dran, in his supplement au Parallele des differentes manieres, &c. 1756, described two new instruments which he had used with much success; a director with a beak like a gorget, which beak was conducted into the groove of the staff, and slid along it into the bladder: the staff being withdrawn, the sulcus of the director was so turned as to face obliquely outwards and downwards, and a bistoury (the blade half an inch broad, and three fourths of an inch long) conveyed along it, served to divide the prostate and neck of the bladder laterally. The gorget was then passed upon the director. The advantage of the director and knife is the size of them, which being less than that of the gorget and knife, admits of their easier and surer introduction. To this narration of his experience M. Le Dran adds some very judicious and pertinent observations, on the necessity of incising the prostate and neck of the bladder.

Pallucci, in a work published at Vienna in 1757, entitled 'La Lithotomie nouvellement perfectionnée,' proposed, after cutting with a small knife through the integuments of the urethra, to fix the point of a trocar in the groove of the staff between the bulb and prostate, and in a groove on the under side of the trocar to conduct the point of the scalpel; then to withdraw the trocar, and cut from above downward very profoundly so as to divide the prostate and the neck. Pouteau, of Lyons, operated with a knife resembling Cheselden's, excepting that the blade was shorter, its back blunt to the point, and its edge more convex†. In 1763 he published a method of operating to prevent cutting the bladder, which he thought objectionable.

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* Phil. Trans. 1752.

† Melanges de Chirurgie, 1760.

It consisted in the disposition of the patient *in a line parallel to the horizon*. His instrument possessed no novelty of principle.

Mr. Bromfield, surgeon to St. George's hospital, used a knife and spoon combined, or what he called a double gorgeret, which he introduced in the same manner as the gorget in common use. The edges of the upper gorget which had a straight handle moved in lateral grooves of the lower, so as to make with it one instrument. A temporary blade was affixed to its left side by a screw. The lower gorget had an angular handle and a beak. They were introduced as one, and having reached the bladder, the upper was withdrawn, and the lower left for the passage of the forceps.

With a view to simplify the operation by diminishing the number of instruments, and to obviate the danger of wounding the rectum, Mr. (afterwards Sir Cæsar) Hawkins, introduced a very capital improvement. This was to give an edge to the director or gorget, which should perform the deep incision, hitherto effected by the knife. It was published in Paris, and sanctioned by the experience of M. Louis, in 1769. The form of this gorget is familiarly known. Its bowl is considerably concave, its base broad, and gradually narrowing to the extremity. The beak rising from the middle, is most eminent at the point, and of the right edge a small space is cutting. We must content ourselves with very briefly enumerating the principal modifications of this instrument, as to detail them would swell this article to a very disproportionate length.

Mr. Benjamin Bell lessens the breadth of the blunt shoulder, and expands the cutting edge so as to divide the prostate more freely. He likewise advises a grooved director instead of a gorget, shaped like the latter at its extremity.

M. Desault reduces the hollow almost to a plane, and makes the breadth of the instrument uniform from the base to the cutting edge. This he limits to an anterior third of the right side, and substitutes for the projecting beak a crest terminating in a straight line at the point, and somewhat nearer to the left side. The handle instead of a lateral inclination, preserves the same direction as the blade.

Dr. Monro devised a blunt gorget set upon a sharp one; the latter introduces the former, and then resigns to it the precedence.

Dr. Jeffray *e contrario* has a sharp set upon a blunt gorget, with precisely the same view. Both are complicated pieces of machinery, and such as would in our opinion, with deference to the Professors, be more creditable to Birmingham and Sheffield than Edinburgh and Glasgow.

Mr. Savigny is the author of a considerable improvement of Hawkins's gorget. This consisted in flattening the edge, and making the beak an elliptic arch, so that the highest part is a portion of the arch instead of the point.

Mr. Cruickshanks's gorget has one-third of the bowl on the left of the beak, and this side projecting one-eighth of an inch beyond the right; the edge forms a bold angle with the beak.

A gorget has been recommended carrying a small button instead of a beak to lock into the groove of the staff, which is contracted in the middle and open at the end. This is a meagre if not a dangerous contrivance, and has been anticipated by Tarinus, Le Cat, and others. An experienced Lithotomist who once tried this apparatus, complained that it exceedingly hampered him in the progress of the operation. Deschamps speaks of a machine, somewhat resembling the double staff of Sir James Earle, invented by Jarda, of Montpellier. We rather wonder that Maillardet let pass the opportunity of a *self-directing Lithotome.* We have heard of 'every man his own barber,' and 'every man his own physician.' Possibly '*Every man his own Lithotomist,*' might ensure an equally popular reception.

Thus far the only essential difference in the construction of the gorget, consisted in the length and shape of the cutting edge. The instrument of consequence lacerated on one side and cut on the other, producing a compound form of wound, refusing to act without the exertion of considerable force, and at the same time acting unequally.

Mr. Cline introduced a most material improvement. In his gorget the left side is done away, the beak being in a line with the same side of the instrument. The right edge forms with the beak an acute angle in a horizontal direction.

In the 9th vol. of the Medical and Physical Journal, Mr. Abernethy has explained the construction of his gorget, which really differs from Mr. Cline's in nothing, but in making the cutting edge to form an angle of 45° with the beak, so as to produce an oblique instead of a horizontal incision. This he conceives to be the most favourable direction for the wound of the prostate. But we would ask Mr. Abernethy, 'whether in his mode the greater proximity of the ramus ischii, does not in a much greater degree expose the artery to danger? and whether, if a large calculus is to be extracted, the room afforded by dilating in this direction is not

less than at any other part of the arch of the pelvis? The very reverse direction, had any improvement on the principle of Mr. Cline been required, would have been much less hazardous, with respect to the artery, have afforded much greater latitude for the extraction of a stone, and have commanded all the advantage of obliquity.

Mr. Astley Cooper gave the following correction of the gorget. The shape nearly resembling the original instrument of Hawkins, only less hollowed, with angular sides cutting to an equal extent from the beak. This is unquestionably, in our opinion, the best form for a cutting gorget. By this construction it enters the bladder with the greatest ease and steadiness, as it requires an equality of pressure, cutting both sides of the gland as it advances, so as to make the wound in the middle of the arch of the pubes. If this be small, the circumference of the prostate is left entire, if large, very large it never need be from its construction, both lobes are divided, but the incision never extends beyond it. Mr. Cooper stated his objects in its construction to be, first, that of avoiding the pudic artery, which is so frequently divided by gorgets that cut on one side only, and second, to give the greatest room for the removal of a stone according to the construction of the pelvic arch. Professor Thomson, we have heard, has recommended a double bistoire caché to act upon the same principle. Its advantage must be that of the knife over the gorget, viz. that it enters without cutting or lacerating and of course without resistance. But the objections to it are those made to the bistoire caché. If we mistake not, it is analogous to the double lithotome of M. Flurant for extracting the stone in women.*

We have now concluded our retrospect of the principal varieties of lithotomes and conductors hitherto published, bearing the sanction either of great names or sound experience; and it will be evident to the most cursory reader, that the principle of both instruments under all their modifications is respectively the same, their differences referring only to form. Of the double edged knife only one edge is employed, the other being turned towards the groove, and consequently the principle of its operation is in every respect the same as that which cuts only on one side. Hence Mr. Cheselden's knife was the simple lithotome of Jacques. If the latter had a blunt dilator, Mr. Cheselden's was again the same. If cutting on one side, as Garan-

geot asserts, it was the gorget of Hawkins. The experiment of Foubert complicates the operation of puncturing the bladder with that of cutting it, and the trocar of Pallucci performing little more than the office of a director, must be considered in common with all pointed instruments, much inferior to those that are beaked, either for retaining or readily moving their situation in the groove of the staff. The bistoire caché has in one respect, and that a most important one, a decided superiority over the cutting gorget, viz. in entering the bladder with all the facility and consequent certainty of a probe. It has another scarcely inferior advantage in cutting as it is withdrawn, which from the small space it occupies in the wound, enables the operator to ascertain in part, the size of the stone before he finishes the aperture for its removal. The objection to it forms the recommendation of the simple bistoury, viz. that it is put in motion by intermediate machinery, and not directly by the hand of the operator; consequently it is not impeded by resistance or the want of it, and it probably cuts in ten cases out of twelve more or less than was necessary or proper. In all covert operations, where the surgeon cannot see the steps he takes, much less retrace them, it is surely requisite that he should feel them. Every one knows how much the suspension of one sense increases the power of another. Lithotomists should operate by tact. He who trusts his operation to the rule and line, should previously ascertain the deviations in his patient from a standard angle of the pubes, diameter of the prostate, and axis of the bladder. All are so far from being made alike, that there is probably as great a variety in pelves as in faces.

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* Melanges de Chirurgie.

Le Cat's knives afforded no security against the danger of slipping: the first, or myrtle leaf was altogether superfluous; the second, or abscess lancet was in fact only a small gorget, as the operator inclined his staff forward at the time it entered. An air of importance is often thrown over a new proposal, which its practical value by no means warrants. "A quoi (says Bertrandi) peuvent servir l'un et l'autre scalpel de M. Lecat? Un chirurgien, qui après avoir fait l'incision des tegumens, ne seroit pas sur d'atteindre promptement jusqu'á la prostate avec l'un ou l'autre scalpel de la figure premiere ou seconde*, ne pourroit certainement pas se promettre d'y atteindre plus facilement avec le scalpel A, et si avec celui-ci même il ne pouvoit pas descendre jusqu'a la prostate, comment pourra-t'on s'imaginer qu'il y parviendra plus sûrement avec le scalpel B?"—Opérations de Chirurgie, p. 550.

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* Alluding to the knives in general use.

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reduced to a level with the arrogant pretensions of ig-
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sprung, and *shaving* is our proper sphere.

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Le Cat's knives afforded no security against the danger of slipping: the first, or myrtle leaf was altogether superfluous; the second, or abscess lancet was in fact only a small gorget, as the operator inclined his staff forward at the time it entered. An air of importance is often thrown over a new proposal, which its practical value by no means warrants. "A quoi (says Bertrandi) peuvent servir l'un et l'autre scalpel de M. Lecat? Un chirurgien, qui après avoir fait l'incision des tegumens, ne seroit pas sur d'atteindre promptement jusqu'á la prostate avec l'un ou l'autre scalpel de la figure premiere ou seconde*, ne pourroit certainement pas se promettre d'y atteindre plus facilement avec le scalpel A, et si avec celui-ci même il ne pouvoit pas descendre jusqu'a la prostate, comment pourra-t'on s'imaginer qu'il y parviendra plus sûrement avec le scalpel B?"—Opérations de Chirurgie, p. 550.

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* Alluding to the knives in general use.

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less than at any other part of the arch of the pelvis? The very reverse direction, had any improvement on the principle of Mr. Cline been required, would have been much less hazardous, with respect to the artery, have afforded much greater latitude for the extraction of a stone, and have commanded all the advantage of obliquity.

Mr. Astley Cooper gave the following correction of the gorget. The shape nearly resembling the original instrument of Hawkins, only less hollowed, with angular sides cutting to an equal extent from the beak. This is unquestionably, in our opinion, the best form for a cutting gorget. By this construction it enters the bladder with the greatest ease and steadiness, as it requires an equality of pressure, cutting both sides of the gland as it advances, so as to make the wound in the middle of the arch of the pubes. If this be small, the circumference of the prostate is left entire, if large, very large it never need be from its construction, both lobes are divided, but the incision never extends beyond it. Mr. Cooper stated his objects in its construction, to be, first, that of avoiding the pudic artery, which is so frequently divided by gorgets that cut on one side only, and second, to give the greatest room for the removal of a stone according to the construction of the pelvic arch. Professor Thomson, we have heard, has recommended a double bistoire caché to act upon the same principle. Its advantage must be that of the knife over the gorget, viz. that it enters without cutting or lacerating and of course without resistance. But the objections to it are those made to the bistoire caché. If we mistake not, it is analogous to the double lithotome of M. Flurant for extracting the stone in women.*

We have now concluded our retrospect of the principal varieties of lithotomes and conductors hitherto published, bearing the sanction either of great names or sound experience; and it will be evident to the most cursory reader, that the principle of both instruments under all their modifications is respectively the same, their differences referring only to form. Of the double edged knife only one edge is employed, the other being turned towards the groove, and consequently the principle of its operation is in every respect the same as that which cuts only on one side. Hence Mr. Cheselden's knife was the simple lithotome of Jacques. If the latter had a blunt dilator, Mr. Cheselden's was again the same. If cutting on one side, as Garan-

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The dilatation performed by M. Le Dran is perhaps the simplest and safest ever devised. From the size of his director and knife they are introduced without exertion; the former being beaked, slides easily through the groove of the staff into the bladder, and being straight, it is hardly possible for the dilating knife to escape from its groove. The objection to his operation is the superfluous extent of his section upwards, requiring the division of the bulb as in the apparatus major; and to this may be added the number of instruments necessary to perform it. But by an imitator of this operation in the present day, both these objections would be remedied. *We* know no occasion for cutting the bulb, or any advantage in passing a blunt spoon.

The inventions of St. Cosmes, Le Cat, Jarda, &c. &c. are all mechanical devices, and as such may deserve attention, but they are *derogatory to the dignity of surgery*. What advantage, what honor is left for the professors of our art, if ingenuity (to call it by too good a name) be accepted as a substitute for information? How are the limits of our knowledge ever to be expanded, if the proud and self-confident superiority of education and talent is reduced to a level with the arrogant pretensions of ignorance and stupidity? Let us but lose the distinction of *intellect* and all is lost. We degenerate into the herd from which we sprung, and *shaving* is our proper sphere.

We shall conclude this article by a short description of the modern plans of operating with the knife alone. Mr. John Bell recommends cutting from behind the prostate, which he exposes by a low and deep incision, with a common scalpel forwards to the membranous part. Mr. Charles Bell has a lateral groove in his staff, and pushes the point of a common scalpel somewhat obliquely into the urethra and into this lateral groove, just before the prostate gland. The fore finger follows the knife, and is to be carried into the bladder on the back of it. Mr. Allan Burns has modified the operation of Mr. John Bell, by introducing a straight director after the plan of Le Dran and Mr. Dease of Dublin, pointing the groove towards the left ramus ischii, perforating the coats of the bladder with the point of a curved knife beyond the prostate, then fixing it in the groove of the straight staff, and withdrawing both instruments together. We wish we could see any advantage possessed by these operations over that with the gorget. The external wound if of such size as to expose the whole of the prostate and a portion of the neck of the bladder, while it adds most uselessly to the number of parts included in

the incision, and to the severity of the operation, must considerably retard the healing of the wound. What must be the size of this wound in a case of enlarged prostate?*

Mr. J. Bell's operation we admit is open enough to the strictures of Mr. Burns†, but we by no means concur with him in thinking his own free from similar objections. "The slipping of the scalpel, (says Mr. Burns) is not the only danger; the knife may remain correctly in the groove, and yet parts may be cut which ought not to have been touched. If the prostate be indurated its division is difficult, it yields before the knife; is pulled low down into the perineum; then the edge of the knife touches the bulb, sinks into its substance, blood is effused, &c." It is true the bulb cannot be injured by Mr. Burns's apparatus, his straight staff protects it; but the induration of the prostate, which we know is sooner lacerated in this case than divided, will cause it either to be so lacerated by the forcible retraction of his two instruments as to form a wound very difficult to heal, or incur the hazard of a much worse accident, namely, the entire separation of the urethra from the bladder at its neck. We know that such an accident may happen. To a certain extent the advice of Pouteau, to avoid as much as possible wounding the bladder, is good. He compares the passage of a stone through the dilated neck of that viscus to the passage of a fœtus through the os tincæ, and though the illustration may not be altogher apposite, it is evident that the laceration of the fibres of either after a lateral incised wound, will take place to an almost indefinite extent. In the removal of large stones however, such a wound is absolutely necessary. But it would perhaps in all cases be better to attempt to leave the bladder entire, since it is easy to dilate it if its resistance to the stone should require it. The prostate unquestionably forms the chief obstruction.

Mr. Gibson of the Manchester Infirmary, a gentleman well known and deservedly esteemed in the surgical world, has for the last seven years practised the operation in private and in a public hospital, with the convex beaked knife represented Fig. 1. in the plate. The previous incision into the membranous part of the urethra is made in the usual way. The beak of the

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knife is then introduced into the groove, and pushed steadily along it till it reaches the cavity of the bladder. From its curved shape the knife divides a portion of the prostate gland as it enters. The staff is now withdrawn, and the knife held firmly while the fore finger of the left hand is conducted along its back, till its last joint is hooked within the cavity of the bladder. This is generally effected without much pressure, since the knife, on its introduction, divides the parts sufficiently to make way for the passage of the finger. The operator now completes the incision of the prostate and bladder. The knife is grasped firmly in the right hand in the position it was introduced. The back of the fore finger of the left hand is turned toward the back of the knife; the palm of the hand is consequently directed towards the patient's right hip. In this posture of parts, the thumb may incline the rectum downwards, and towards the right side. The prostate and neck of the bladder are firmly pressed by the before-mentioned finger towards the right side of the pelvis, whilst the knife, carried in a contrary direction, its edge inclined a little obliquely downwards, divides the parts in some measure put upon the stretch, by a slight degree of motion. As soon as the left side of the prostate is divided, which is easily known by a little experience, the middle finger may be introduced to ascertain the size of the opening, and what portion of the bladder it may be necessary to divide. If the division of the parts contiguous to the bladder has not been made with sufficient freedom at the commencement of the operation, it may be completed on withdrawing the knife, so as to make the external correspond with the internal incision. The finger will be a sufficient conductor for the forceps. The advantages of this operation are, 1st. The little resistance the knife encounters on its introduction, and hence little liability to slip from the groove of the staff. 2dly, Having the fore finger as a guide, so that the operator can feel and determine accurately the part he is dividing. During the incision of the prostate for instance, the resistance offered to the knife is easily recognised; and the cessation of that resistance when the incision has reached the bladder. 3dly, The circumstance of the bladder holding a larger or smaller quantity of urine being perfectly immaterial, at the time the incision is made. 4thly, The pudic artery may with great certainty be avoided; and if it be necessary from the size of the stone, to make the incision very extensive, the operator is in possession of an instrument with which he can easily accommodate the size of the opening in the bladder to the size of the stone; a circumstance of the greatest importance.

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Mr. Astley Cooper of Guy's Hospital has cut several patients during the last twelvemonth with the knife alone. In seven cases he used the common scalpel with a button at its extremity, of which Fig. 5, is an outline; in an eighth he employed a probe pointed bistoury. Whether he assigns a preference to the gorget or the knife, we are unable to state with certainty. To this simple form of instument however, may be reduced all the varieties of the gorget from the time of Jacques to the present day. They differ in nothing but the length of the cutting edge.

Fig. 2. represents the knife successfully used by Mr. Thomas Blizard of the London Hospital. Its beak is introduced after the first incision by the side of the left fore-finger along the groove of the staff, and having fairly entered the cavity of the bladder, the operator allows the beak to quit the groove, and dilates in a direction a little obliquely downwards in the act of withdrawing it. It differs from the former in not acting as it enters. We conceive however, that the straight blade is scarcely equal to perform a dilatation of sufficient extent in a single movement, and that the greater extent of a convex edge would ensure to it a greater facility and command. The beak it must be observed is set off from the line of the instrument, and the extremity of the blade is blunt for the space of an inch. This enables it more easily to quit the groove, and provides against the danger of wounding the bladder.

Professor Thomson states the usual objections to the gorget, and that they are not speculative, requires no proof more conclusive than that they are universally admitted. The danger in Mr. Cheselden's operation was not from force employed to introduce his instrument, for this it did not require; nor from the danger of its slipping, for this could hardly happen where no force was exerted; nor from the uncertainty which attends the use of the gorget, that the size of the wound will be insufficient for the removal of the stone, for the size of the one could be adapted to that of the other; but from the possibility of wounding the rectum, especially if much dilated, since his incision was directed towards it. Hence Dr. Thomson proposes to introduce a straight staff into the groove of the curved staff, and push it into the bladder.

"The curved staff is then to be withdrawn, and the surgeon laying hold of the handle of the straight staff with his left hand, and turning the groove upwards and a little outwards, presses the back of it downwards towards the right tuber ischii, and holds it steadily in that position. The

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The point of a straight backed scalpel being now introduced into the groove of the staff, with its cutting edge inclined *upwards* and a little outwards, is to be pushed gently forwards into the bladder. The size of the scalpel need only be such as will make a wound in the prostate gland and neck of the bladder, sufficiently large to admit the forefinger of the left hand. The scalpel being removed, this finger is to be introduced into the bladder through the wound which has been made, and the staff may then be withdrawn. With the finger the surgeon endeavours to ascertain the size and situation of the stone. If after this examination he judges the incision in the neck of the bladder o be too small for the easy extraction of the stone, he next introduces nto the bladder a straight probe-pointed bistoury, with its side close o the fore part of his finger, and its cutting edge upwards. By turnng this edge towards the left side, and by keeping the point of his inger always beyond the point of the bistoury, he may safely divide n the direction of the first incision, as much of the prostate gland nd neck of the bladder as he shall deem necessary."

If the stone be very large and the rectum not over dilated, the cision may be made outwards and downwards. Dr. Thomson ppears to rest upon the advantage of regulating the action of the nife by that of the finger, and for the reasons before assigned, e esteem the use of the finger previous to making the incision s well as in the act of making it, the greatest advantage which he knife commands. With regard to the direction of the inision, this we think, should in all casesbe obliquely *downwards*; ecause if made with sufficient obliquity, the rectum is always ecure from injury; because the wound of the perineum in this fords a larger diameter than in the reverse direction; and lastly, ecause, as we have before said, it gives greater security to the tery, and greater latitude for dilatation if that should be necesry. Further, if the wound be made upwards, a small and oveable calculus will lie so much below the forceps as not to seized without difficulty, and a stone of any size will hitch ainst the ramus pubis, and effectually foil the efforts at exaction.

We do not mean to express a decided opinion in favour any particular form of Lithotome. The objections to e gorget are well known, and we will add, well founded. But the knife objections exist, which if this instrument should me into general use, will speedily decide the question. It plays too much the power of the anatomist not to expose most rfully the weakness of the mechanic. Hence there will be, d naturally, a backwardness to employ it. Though it was dled most skilfully near a century ago, it is still a *feat*, and l be so acknowledged. We have ventured to prefer, not

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The dilatation performed by M. Le Dran is perhaps the simplest and safest ever devised. From the size of his director and knife they are introduced without exertion; the former being beaked, slides easily through the groove of the staff into the bladder, and being straight, it is hardly possible for the dilating knife to escape from its groove. The objection to his operation is the superfluous extent of his section upwards, requiring the division of the bulb as in the apparatus major; and to this may be added the number of instruments necessary to perform it. But by an imitator of this operation in the present day, both these objections would be remedied. *We* know no occasion for cutting the bulb, or any advantage in passing a blunt spoon.

The inventions of St. Cosmes, Le Cat, Jarda, &c. &c. are all mechanical devices, and as such may deserve attention, but they are *derogatory to the dignity of surgery*. What advantage, what honor is left for the professors of our art, if ingenuity (to call it by too good a name) be accepted as a substitute for information? How are the limits of our knowledge ever to be expanded, if the proud and self-confident superiority of education and talent is reduced to a level with the arrogant pretensions of ignorance and stupidity? Let us but lose the distinction of *intellect* and all is lost. We degenerate into the herd from which we sprung, and *shaving* is our proper sphere.

We shall conclude this article by a short description of the modern plans of operating with the knife alone. Mr. John Bell recommends cutting from behind the prostate, which he exposes by a low and deep incision, with a common scalpel forwards to the membranous part. Mr. Charles Bell has a lateral groove in his staff, and pushes the point of a common scalpel somewhat obliquely into the urethra and into this lateral groove, just before the prostate gland. The fore finger follows the knife, and is to be carried into the bladder on the back of it. Mr. Allan Burns has modified the operation of Mr. John Bell, by introducing a straight director after the plan of Le Dran and Mr. Dease of Dublin, pointing the groove towards the left ramus ischii, perforating the coats of the bladder with the point of a curved knife beyond the prostate, then fixing it in the groove of the straight staff, and withdrawing both instruments together. We wish we could see any advantage possessed by these operations over that with the gorget. The external wound if of such size as to expose the whole of the prostate and a portion of the neck of the bladder, while it adds most uselessly to the number of parts included in

the incision, and to the severity of the operation, must considerably retard the healing of the wound. What must be the size of this wound in a case of enlarged prostate?*

Mr. J. Bell's operation we admit is open enough to the strictures of Mr. Burns†, but we by no means concur with him in thinking his own free from similar objections. "The slipping of the scalpel, (says Mr. Burns) is not the only danger; the knife may remain correctly in the groove, and yet parts may be cut which ought not to have been touched. If the prostate be indurated its division is difficult, it yields before the knife; is pulled low down into the perineum; then the edge of the knife touches the bulb, sinks into its substance, blood is effused, &c." It is true the bulb cannot be injured by Mr. Burns's apparatus, his straight staff protects it; but the induration of the prostate, which we know is sooner lacerated in this case than divided, will cause it either to be so lacerated by the forcible retraction of his two instruments as to form a wound very difficult to heal, or incur the hazard of a much worse accident, namely, the entire separation of the urethra from the bladder at its neck. We know that such an accident may happen. To a certain extent the advice of Pouteau, to avoid as much as possible wounding the bladder, is good. He compares the passage of a stone through the dilated neck of that viscus to the passage of a fœtus through the os tincæ, and though the illustration may not be altogher apposite, it is evident that the laceration of the fibres of either after a lateral incised wound, will take place to an almost indefinite extent. In the removal of large stones however, such a wound is absolutely necessary. But it would perhaps in all cases be better to attempt to leave the bladder entire, since it is easy to dilate it if its resistance to the stone should require it. The prostate unquestionably forms the chief obstruction.

Mr. Gibson of the Manchester Infirmary, a gentleman well known and deservedly esteemed in the surgical world, has for the last seven years practised the operation in private and in a public hospital, with the convex beaked knife represented Fig. 1. in the plate. The previous incision into the membranous part of the urethra is made in the usual way. The beak of

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knife is then introduced into the groove, and pushed steadily along it till it reaches the cavity of the bladder. From its curved shape the knife divides a portion of the prostate gland as it enters. The staff is now withdrawn, and the knife held firmly while the fore finger of the left hand is conducted along its back, till its last joint is hooked within the cavity of the bladder. This is generally effected without much pressure, since the knife, on its introduction, divides the parts sufficiently to make way for the passage of the finger. The operator now completes the incision of the prostate and bladder. The knife is grasped firmly in the right hand in the position it was introduced. The back of the fore finger of the left hand is turned toward the back of the knife; the palm of the hand is consequently directed towards the patient's right hip. In this posture of parts, the thumb may incline the rectum downwards, and towards the right side. The prostate and neck of the bladder are firmly pressed by the before-mentioned finger towards the right side of the pelvis, whilst the knife, carried in a contrary direction, its edge inclined a little obliquely downwards, divides the parts in some measure put upon the stretch, by a slight degree of motion. As soon as the left side of the prostate is divided, which is easily known by a little experience, the middle finger may be introduced to ascertain the size of the opening, and what portion of the bladder it may be necessary to divide. If the division of the parts contiguous to the bladder has not been made with sufficient freedom at the commencement of the operation, it may be completed on withdrawing the knife, so as to make the external correspond with the internal incision. The finger will be a sufficient conductor for the forceps. The advantages of this operation are, 1st. The little resistance the knife encounters on its introduction, and hence little liability to slip from the groove of the staff. 2dly, Having the fore finger as a guide, so that the operator can feel and determine accurately the part he is dividing. During the incision of the prostate for instance, the resistance offered to the knife is easily recognised; and the cessation of that resistance when the incision has reached the bladder. 3dly, The circumstance of the bladder holding a larger or smaller quantity of urine being perfectly immaterial, at the time the incision is made. 4thly, The pudic artery may with great certainty be avoided; and if it be necessary from the size of the stone, to make the incision very extensive, the operator is in possession of an instrument with which he can easily accommodate the size of the opening in the bladder to the size of the stone; a circumstance of the greatest importance.

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Mr. Astley Cooper of Guy's Hospital has cut several patients during the last twelvemonth with the knife alone. In seven cases he used the common scalpel with a button at its extremity, of which Fig. 5, is an outline; in an eighth he employed a probe pointed bistoury. Whether he assigns a preference to the gorget or the knife, we are unable to state with certainty. To this simple form of instument however, may be reduced all the varieties of the gorget from the time of Jacques to the present day. They differ in nothing but the length of the cutting edge.

Fig. 2. represents the knife successfully used by Mr. Thomas Blizard of the London Hospital. Its beak is introduced after the first incision by the side of the left fore-finger along the groove of the staff, and having fairly entered the cavity of the bladder, the operator allows the beak to quit the groove, and dilates in a direction a little obliquely downwards in the act of withdrawing it. It differs from the former in not acting as it enters. We conceive however, that the straight blade is scarcely equal to perform a dilatation of sufficient extent in a single movement, and that the greater extent of a convex edge would ensure to it a greater facility and command. The beak it must be observed is set off from the line of the instrument, and the extremity of the blade is blunt for the space of an inch. This enables it more easily to quit the groove, and provides against the danger of wounding the bladder.

Professor Thomson states the usual objections to the gorget, and that they are not speculative, requires no proof more conclusive than that they are universally admitted. The danger in Mr. Cheselden's operation was not from force employed to introduce his instrument, for this it did not require; nor from the danger of its slipping, for this could hardly happen where no force was exerted; nor from the uncertainty which attends the use of the gorget, that the size of the wound will be insufficient for the removal of the stone, for the size of the one could be adapted to that of the other; but from the possibility of wounding the rectum, especially if much dilated, since his incision was directed towards it. Hence Dr. Thomson proposes to introduce a straight staff into the groove of the curved staff, and push it into the bladder.

"The curved staff is then to be withdrawn, and the surgeon laying hold of the handle of the straight staff with his left hand, and turning the groove upwards and a little outwards, presses the back of it downwards towards the right tuber ischii, and holds it steadily in that position. The

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The point of a straight backed scalpel being now introduced into the groove of the staff, with its cutting edge inclined *upwards* and a little outwards, is to be pushed gently forwards into the bladder. The size of the scalpel need only be such as will make a wound in the prostate gland and neck of the bladder, sufficiently large to admit the forefinger of the left hand. The scalpel being removed, this finger is to be introduced into the bladder through the wound which has been made, and the staff may then be withdrawn. With the finger the surgeon endeavours to ascertain the size and situation of the stone. If after this examination he judges the incision in the neck of the bladder to be too small for the easy extraction of the stone, he next introduces into the bladder a straight probe-pointed bistoury, with its side close to the fore part of his finger, and its cutting edge upwards. By turning this edge towards the left side, and by keeping the point of his finger always beyond the point of the bistoury, he may safely divide in the direction of the first incision, as much of the prostate gland and neck of the bladder as he shall deem necessary."

If the stone be very large and the rectum not over dilated, the incision may be made outwards and downwards. Dr. Thomson appears to rest upon the advantage of regulating the action of the knife by that of the finger, and for the reasons before assigned, we esteem the use of the finger previous to making the incision, as well as in the act of making it, the greatest advantage which the knife commands. With regard to the direction of the incision, this we think, should in all casesbe obliquely *downwards*; because if made with sufficient obliquity, the rectum is always secure from injury; because the wound of the perineum in this affords a larger diameter than in the reverse direction; and lastly, because, as we have before said, it gives greater security to the artery, and greater latitude for dilatation if that should be necessary. Further, if the wound be made upwards, a small and moveable calculus will lie so much below the forceps as not to be seized without difficulty, and a stone of any size will hitch against the ramus pubis, and effectually foil the efforts at extraction.

We do not mean to express a decided opinion in favour of any particular form of Lithotome. The objections to the gorget are well known, and we will add, well founded. But to the knife objections exist, which if this instrument should come into general use, will speedily decide the question. It displays too much the power of the anatomist not to expose most fearfully the weakness of the mechanic. Hence there will be, and naturally, a backwardness to employ it. Though it was handled most skilfully near a century ago, it is still a *feat*, and will be so acknowledged. We have ventured to prefer, not

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It is customary in cases where much division of opinion exists, to determine the claim to preference by the ordeal of experience. In the present question a recurrence to this test will lead us to no satisfactory conclusion. It will teach us not so much the unfitness of any one instrument as the fitness of all. Mr. Hunter in the confidence of his skill, demanded only a knife and probe to traverse the vast labyrinth of the living body. What has been done we know, and it is enough to prove beyond all controversy, the futility of attempting to set bounds to what may be. The success of James, and of every original operator since his time, (if by such a name we may dignify the contriver of an instrument) establishes incontestibly, that lithotomy may be performed with the most complicated, and the most simple lithotome, the roughest and the most polished steel. But how much let us ask, has the since enormous expenditure of mechanical contrivance advanced the number of our cures over those of Cheselden? What real improvement has been contributed by the art of the surgeon? For the *cutler* we provide an honourable exception. On the contrary it cannot be denied, that the casualties which have since attended the operation in the hands of most responsible and experienced surgeons, have been such and so frequent as seemed imperiously to call for an alteration in the circumstances of its performance; and yet who could have the effrontery to impute to an hospital surgeon the disgrace of laying open the rectum, or dividing the pudic artery. Surely no one! The instruments, and they alone, were in fault. We do not withhold our homage to new inventions (for God knows they are rare,) nor do we join in the hierarchical cry of "Down with innovation," the establishment is in danger; on the contrary, we are sticklers for the public spirit of every projector who runs the risk of offending his reviewer: but sure we are, that so much depends upon men and so little upon their weapons, that the same accidents will recur, and the same evasions be resorted to, so long as stones are formed and surgery is practised. In short, we agree entirely with M. Louis, Une operation ne peut pas être uniquement assujèttie à la mecanique d'un instrument, mais *l'habileté et les lumières de l'operateur en feront toujours la principal sûreté.*

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We have heard it regretted that Dr. Thomson, for whose extensive and profound erudition in the literature of his profession we desire to express our unfeigned respect, should content himself with the reputation of an editor; that he should not rather resign to a pupil the credit of a publication in its nature so little calculated to advance his own. That it is so, the professor seems to be well aware in the selection of his admirable motto: "Créer des moyens operatoires nouveaux, c'est souvent plus ajouter à sa gloire qu'aux progrès de la chirurgie; en ressusciter d'anciens, c'est au contraire faire peu pour soi, et beaucoup pour l'art*."

But indeed, when we consider how many novelties of the present day turn out to have been the novelties of days long past, we cannot but acknowledge our gratitude to those disinterested inquirers, who prefer to an ostentatious display of their own pretensions, the merits of their ancestors, and the glory of their art. We know too well the talents and opportunities of Dr. Thomson, the ardour which he has himself displayed, and with which he has inspired others in the prosecution of his labours as a teacher, to entertain a doubt of the sufficiency of his future claim, and that at no distant period, to the particular respect and gratitude of the medical profession. In the mean time, his knowledge will ensure our confidence, and his motives our applause.

ART. VII. *A Letter to the Commissioners of Military Inquiry, containing Animadversions on some parts of their fifth Report, and an Examination of the Principles on which the Medical Department of Armies ought to be formed.* By Edward Nathaniel Bancroft, M. D. 8vo. 3s.

A Letter to the Commissioners of Military Inquiry, in reply to Dr. E. N. Bancroft. By James M'Gregor, M. D. 8vo. 2s. London, Murray, 1808.

IT is curious to observe how partial is the interest excited by the greater number of human events, and how trifling a distance they penetrate into the bosom of society from the point whence they radiate. A new doctrine will awaken the slumbering activity of the medical profession, and be wholly unknown to the rest of the world. The barristers of the metropolis may be talking of the eloquence of one of their rising colleagues whose name would sound strange out of the pale of their profession. A vehement discussion may occupy the theological world, while the subject of the dispute and the names of the disputants may be unknown to many a village curate. It is probable therefore that not a few of our medical readers have been so thoroughly occupied by the active or the speculative employments of their profession, in visiting their patients, framing their hypotheses, and writing their prescriptions, that they have never heard of the Commissioners of Military Inquiry, cannot conceive any possible relation between such personages and physic, and will be surprised to see the publications at the head of this article, introduced into a Medical Review.

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* Œuvres Chirurgicales de Desault.

A certain number of men under the name of commissioners, were appointed by government to examine the state of the army, to report its imperfections, and to propose such remedies as appeared proper. Among the other objects of their attention, the commissioners scrutinized, censured, and proposed to correct the medical arrangement of the army. Some of those censures and proposals have given rise to the Letter of Dr. Bancroft and the Reply of Dr. M'Gregor. The subjects of these censures, proposals, letters, and replies, although less connected with medicine than a dissertation on fever, yet can be properly discussed only by medical men, and properly reviewed only by medical reviewers. This is our apology for noticing these publications.

The commissioners in giving advice about the medical arrangements of the army were acting as counsellors, on a subject in which their knowledge was scanty. The chances of bad advice were therefore considerable. To make up for their want of information they proceeded to read medical books and examine medical men. Here the chances of error were almost as great as those of truth. The watering places of this island abound with a certain description of men who are very curious and somewhat sickly, and who study medicine for want of a better occupation: by reading Buchan and torturing the physician during his morning visit with a multitude of troublesome questions, they are enabled to talk nonsense, and become very vain of their medical acquirements. With regard to medical knowledge, the commissioners and these accomplished gentlemen stand in the same relation. Of all counsellors half-informed ones are the worst. If they seek information from others they are liable to be deceived. Nothing is more easy than to make an erroneous opinion very clear and satisfactory to an ignorant person. The farmers of Essex sow by the broad cast—of Norfolk by the drill—those of Yorkshire plough with six horses, those of East Lothian with two. Each abuses the practice which he does not follow, and each is able to convince an uninformed inquirer that his own is the wisest.

The controversy contained in the above mentioned pamphlets relates chiefly to two topics of advice, to preferring army surgeons to regular physicians for places of high medical responsibility in the army, and to the discontinuance of general hospitals. The commissioners prefer army surgeons, 1st, because men who have passed through all the gradations of medical rank must be best acquainted with army diseases; 2dly, because surgeons who have been educated to all branches of the profession are more skilful than physicians who have been educated only to a part; and, 3dly, because the physician who was once an hospital mate must be well acquainted with the tricks and deceptions of the soldiers, and thus be more able to distinguish the real from the feigned invalid.

The first and last of this trio of reasons hardly require notice. The slight difference which exists between the diseases of a camp and a city, would scarcely be felt as a difficulty and would certainly be soon overcome by any well informed practitioner. The only reason which demands examination is that which puts the education of a surgeon above that of a physician. The commissioners are not aware of the kind of men they have elevated so high. The surgeons will be disposed to stare with astonishment at their unexpected importance.

A raw lad, fresh from the country, comes up to London to finish his education by attending our lectures and hospitals. This accomplished youth beside the art of rolling a pill and labelling a phial, can draw teeth, and dress a blister. He arrives in London, calls on the lecturer to whom his master has given him a letter, and takes his ticket, promises industry, makes his bow and retires. He is to spend only six months in our expensive metropolis. Time flies swiftly. Studious habits are acquired slowly. Christmas is come, and half the allotted period wasted in the multifarious occupations of procuring his lodgings, uncording his boxes, putting his clothes in the drawers, arranging his books neatly on the shelves, and as he calls it "getting settled." He must attend the lectures on medicine, materia medica, chemistry, midwifery, anatomy and surgery. He must dissect, attend the demonstrations, run round the hospital at the heels of the surgeon, and be very assiduous in the general squeeze to get a peep at the patient. He must moreover see all the operations, copy the scrawled, unintelligible, and often inaccurate notes of his lectures, and read some of the most concise books on the subjects of his studies. Even his nights are sometimes disturbed by the loud knocks of the porter calling him to a labour. The multiplicity of his studies dissipates his attention, and what his mind receives one hour is washed away the next.

Month after month rolls on; the winter is past and gone: if he has spent it with industry, he has heard much, understood little, remembered less, been bewildered by a crowd of objects, and just begins to see through the darkness which enveloped him: if, on the contrary, he has frittered it away in idleness, which is most commonly the case, he has lost the little information, and the many steady habits which he brought from his master's house; he has learned to wear a fashionable coat, high-heeled boots, and a pig-tail; he presents himself at the College, with an empty head and a palpitating heart, passes, to the astonishment of his companions, enters the army, and undertakes the preservation of the lives of hundreds. The service is not the school where ignorance or indolence is corrected. His medical library must necessarily be small, because that which belongs to a soldier must be at once portable and cheap. He joins his regiment and becomes intimate with his comrades. The habits of the army are inimical to study and reflection. Sloth is contagious. He cannot sit down to read, or as he would phrase it, to fag. If he does, his tent or his barrack-room is invaded by his boisterous companions. The morning passes away in lounging, the afternoon and evening are spent in the mess-room, and the day closes with riot and debauchery. Indolence thus undermines his activity, his patients are neglected because he loses those regular and reflecting habits which are necessary to make the practical duties of his profession interesting, and what his studies left imperfect his opportunities for observation fail to supply. Such are the men whom, for a minute and scientific knowledge of their profession, the commissioners have preferred to those who have grown up in the solitude, the stillness, the studious habits, the methodical observation, the scientific pursuits, and the vast libraries of an university. If the commissioners were medical men as we are, knew the state of medical knowledge in the service as we do, and could view the subject with impartiality as we can, they would be as deeply sensible as ourselves of the gross error which they have committed. Because surgeons usually enter the army with very imperfect knowledge, because during their continuance in the service they are not likely to augment it much, because the regular physician generally speaking carries to the bed-side a much larger stock of the collected knowledge of the profession, because out of ten cases of disease not more than one requires the aid of surgery, and, because the introduction of men of learning in their profession is likely to diminish the general ignorance of the medical officers of the army, we are clearly of opinion not only that the appointment of regular physicians ought to be continued, but that they ought to be introduced into the service in much larger numbers than they have been.

Our limits will not permit us to go into the question of the utility of general hospitals. We have no satisfactory data upon which to form an opinion, nor are the commissioners in this respect better supplied than ourselves. They find that on the Continent, and during the years 1794 and 1795, the mortality of the general was greater than that of the regimental hospitals, and this superior mortality is referred to something peculiarly destructive in the constitution of a general hospital, and not to a hundred other causes to which it may as well be attributed. The mortality in the clinical is always greater than that in the common wards of our hospitals. No one however, would attribute this to any thing pernicious either in the air or in the treatment of these wards, but to the fact that they purposely receive the worst cases. In this part of their labours the commissioners have been particularly rash, because they have proceeded on information particularly unsatisfactory. There are some situations, as where an army is obliged to make very frequent and very rapid movements, in which it seems to us that a general hospital is indispensable. We are quite willing to allow that the errors which we have been pointing out arose wholly from ignorance, and that the commissioners have acted no wiser because they knew no better; that in searching for the information which they wanted they might have drawn it from clearer sources than the publications of Dr. Jackson.

At the commencement of Lord St. Vincent's administration which, beside its other high merits, was marked by an active hostility to every thing in the shape of laziness and dishonesty—at the commencement of this administration every part of the navy was overrun with abuses, and the hospitals among the rest. The superior surgeons were careless, the inferior ignorant, and the menials thievish. Atrocious examples were discovered of the sick being neglected and the government plundered. In consequence of this disclosure of abuses, his lordship instituted a rigid inspection of all the naval hospitals of the kingdom, and, as he was aware that the efficacy of the scrutiny almost wholly depended on the skill and activity of the person entrusted with the office, he selected Dr. Baird, a man distinguished from his medical brethren in the service by his extensive professional knowledge, and particularly remarkable for an unfatiguing activity of mind, which rendered him peculiarly adapted to the situation for which he was chosen. The effects which might have been expected from the measures of so vigilant a ruler, and the labours of so active an inspector followed in a considerable degree; parsimony from the profuse, attention from the careless, activity from the indolent, and abuse from the interested. If Dr. B. had continued unmolested in his exertions, it is impossible to say to what a degree the system would have been cleared of the foul weeds of ignorance, extravagance, indolence, and peculation. The commissioners would have done well to have studied the regulations of Lord St. Vincent and the reports of Dr. Baird, rather than the lucubrations of Dr. Jackson. They should have taken a leaf from the volumes of experience, and have asked advice from those who had gone before them in a single career. The prudent traveller before he visits an unknown country, studies writers who have explored it before him.

Our limits compel us to take leave of the commissioners, who, we hope, will learn from their past errors future circumspection. As we are not likely to have a better opportunity we take the present one, of saying that the medical concerns of the navy demand the serious attention of government. The management of this department resides almost wholly in the hands of one man, from whom, in proportion to the responsibility of his situation we have a right to expect ability and knowledge.—Let government look to it seriously.

We are not acquainted with the character of Dr. Harness, but from the importance of his office and the wisdom of our rulers, we are naturally led to conjecture, that he has been bred in universities, and practised in hospitals, and that his mind has been trained by science and matured by observation.

ART. VIII. *Essays on the Morbid Anatomy of the Human Eye.* By James Wardrop, Fellow of the Royal College of Surgeons, of the Royal Medical and Chirurgical Societies, and one of the Surgeons of the Public Dispensary of Edinburgh, Edin. Constable and Co. London, Murray, 1l. 4s. pp. 159. 1808.

THE pathological doctrines of Bichat are likely to produce a considerable effect on the future investigations and opinions of English surgeons. The principles of the French physiologist are thus opened in the commencement of his Anatomie Generale.

Every animal is an assemblage of different organs, each executing a function. The organs are particular machines, included in the general machine or individual. These particular machines are themselves composed of several textures, which differ in nature, and constitute the elements of these organs.

Chemistry has its simple bodies, such as caloric, light, hydrogen, oxygen, carbon, azot, phosphorus, &c. forming by the different combinations of which they are susceptible, compound bodies. In the same way anatomy has its simple textures, which by their combinations form the organs. These textures are the cellular, the nervous of animal life, which consists of the nerves, of the senses, and voluntary motion, the nervous of organic life, which consists of the nerves not employed in the senses or voluntary motion, the arterial, the venous, the exhalent, that of the absorbents and their glands, the osseous, the medullary, the cartilaginous, the fibrous, the fibro-cartilaginous, the muscular of animal life, or the voluntary muscular, the muscular of organic life, or the involuntary muscular, the mucous, the synovial, the glandular, that of the cutis, that of the epidermis, and that of the hair. These are the true organic elements of the body. In whatever way they are disposed their nature is constantly the same; as in chemistry, the simple bodies never vary, whatever may be the compositions which they concur to form. These are the organic elements of man. In giving to each system a different organic arrangement, nature also endues it with different properties, as sensibility to the nerves and contractility to the muscles.

Much has been said of the proper life of each organ, which is nothing more than the particular character distinguishing the assemblage of the vital properties of one organ from the assemblage of the vital properties of another organ. Before these properties had been analysed with vigour, it was obviously impossible to form a precise idea of this proper life; but as organs are composed of simple textures differing from one another, it is evident that the idea of proper life can only be applied to these simple textures, and not to the organs themselves.

Thus the stomach is composed of various textures, the serous, the organic muscular, the mucous, the arterial, the venous, &c. If you attempt to describe, in a general way, the proper life of the stomach, it will be obviously impossible to form a precise and rigorous idea. The mucous surface is so different from the serous, both from the muscular, that to associate them in one common consideration is to confound all. When we study a function, we must consider in a general way the compound organ which performs it, but when we wish to know the properties and the life of this organ, we must absolutely decompose it.

These considerations bring us to important consequences relative to diseases which are local, for those, which like the greater number of fevers attack almost at the same time the whole body, cannot be much illuminated by the anatomy of systems. Since diseases are only alterations of the vital properties, and as each texture differs from others in its properties, it is evident that it must differ also in its diseases. In every organ composed of different textures, one may be diseased whilst the others remain unaffected. This is what happens in the greater number of cases. Nothing is more rare than diseases of the cerebral pulp, nothing more common than inflammation of the arachnoid coat which envelopes it. Most commonly one membrane only is diseased in the eye, the others preserving their ordinary mode of vitality.

In spasms or paralysis of the muscles of the larynx, the mucous surface remains untouched, and on the contrary the muscles enjoy their ordinary functions in catarrhal affections of this surface, &c. &c. I believe that the more we observe diseases, and open dead bodies, the more we shall be convinced of the necessity of considering local diseases not with regard to the compound organs which they scarcely ever attack throughout, but with regard to their different textures, which are almost always affected individually.

When diseases are sympathetic they follow the same laws as when they arise from a direct cause. When in the stomach the muscular fibres contract from the influence of another organ and produce vomiting, these fibres alone have received the influence which extends neither to the serous nor to the mucous surfaces, which if affected, would be the seat of a sympathetic exhalation or secretion. I might collect a crowd of other examples to prove that it never is such or such an organ which smpathises throughout, but only such or such a texture in these organs. Let us see what happens in the concomitant fevers of the different phlegmasiæ. Those of the mucous are commonly very trifling, those of the serous always intense, those of the cutaneous have the particular character of appearing some days before the eruption. If you run over attentively those which accompany the inflammation of all the systems, you will find as many differences as there are different characters in the systems.

We must not exaggerate however this independance. In many chronic diseases all the parts of the same organ alter by degrees, and on opening the body the whole of the organ appears affected, although one only of its textures was originally diseased. Since each organized texture has a particular uniform disposition, since, whatever may be its situation it has the same structure and the same properties, it is evident that its diseases ought to be throughout the same. The serous texture belongs to the brain in the arachnoid coat, to the lungs in the pleura, to the heart in the pericardium, to the intestines in the peritoneum. In all it is inflamed in the same manner; in all it is subject to dropsies. Whatever may be the organ included in the mucous texture its diseases bear in general the same character.

In this way Bichat broaches his peculiar pathological principles. In the present work the author professes to employ these principles in investigating the diseases to which the eye is subjected and to examine the morbid alterations in structure, not of the organ in the gross, but of the textures in particular. This plan gives rise to much division and arrangement of subject, and by augmenting the variety of the disquisitions increases the bulk of the work. Thus the volume which lies before us treats only of the diseases of the cornea; whilst those of the iris, the humours, the retina, the choroid and sclerotic coats, the conjunctiva, the cellular membrane, the tarsi, the palpebræ, the lachrymal gland, the lachrymal caruncle, remain to be discussed in future treatises.

The transparent cornea consists of three distinct parts. Its external surface is formed by a portion of the tunica conjunctiva; its internal by a portion of the membrane, which envelopes the aqueous humour; its intermediate substance consists of cellular lamellæ, filled by a colourless and transparent fluid. The cornea, although it is peculiar in its form and appearance, consists in a considerable degree, of component parts similar to those which are found in other parts of the body. Thus the tunica conjunctiva possesses the properties of a mucous membrane, and the tunic of the aqueous humour those of a serous membrane. These two membranes not only enjoy the properties but are subject to the diseases of the membranous class, to which they belong. The cornea therefore is subject on its external surface to the diseases of mucous, and on its internal to the diseases of serous membranes, besides other affections peculiar to the cornea itself.

Mr. Wardrop begins his morbid delineations with a description of inflammation. Although inflammation sometimes spreads throughout all the textures which compose the cornea, yet sometimes it is confined to one. This affection therefore, is divided by our author into that of the conjunctiva of the cornea, that of the proper substance of the cornea, and that of the internal membrane of the cornea. The following is the description of inflammation, as it appears in the conjunctiva of the cornea.

"It is by no means unusual to observe, in those who have had long or repeated attacks of inflammation of the conjunctiva covering the sclerotic coat, that a considerable degree of obscurity and vascularity extends from it over the cornea. This took place in many of our troops who suffered from ophthalmia in Egypt, and who have, since their return home, been subject to frequent inflammatory attacks in their eyes. Many cases have also occurred where the same effect was produced by the ophthalmia which has been so frequent among the troops in this country. The obscurity of the cornea, in the cases to which I allude, appeared to be confined to the external surface. Red vessels became ramified upon its surface, and they were much more numerous in proportion to the degree of opacity, than they ever are in the *speck* of the cornea. I have observed, in a variety of cases of ophthalmia in this country, where the conjunctiva covering the sclerotic coat was attacked with inflammation, that red vessels branched out from a particular portion of the affected part, and extended a little way over the edge of the cornea; but these vessels generally disappeared when the more violent inflammatory symptoms were removed. In all these cases, however, the inflammation commenced and extended from the conjunctiva, covering the white of the eye, to that part of it which covers the cornea. But in other instances, the inflammation first appears in the conjunctiva, covering the cornea, and is almost entirely confined to it alone. When this takes place, it very much resemble a *speck* on a superficial examination, but the commencement, progress, and final termination of these two diseases are very different. Inflammation of the conjunctiva of the cornea begins at the circumference, and gradually extends from this over the surface of the cornea, whereas a *speck* commonly commences at the centre of the cornea, or at some distance from its circumference, and extends in every direction from that point. In the inflammation of the conjunctiva covering the cornea, the opaque part becomes elevated above the natural surface of the cornea, and is highly vascular; whereas, in *speck*, the form of the cornea remains unaltered; and although one or more vascular trunks advance to the speck, yet the distribution of their branches becomes in general imperceptible.

"Inflammation of the conjunctiva of the cornea may also be mistaken for a *pustule* of that membrane; for in the latter disease, the conjunctiva adjacent to the pustule is sometimes distinctly vascular, and elevated above its natural surface. But if the progress of the two affections be attentively observed, they will be found to be different; for a pustule of the cornea, like a speck, begins at one point, extends from it as a centre, and most frequently terminates in an *ulcer*.

"The most remarkable symptom of inflammation of the external covering of the cornea, is the appearance of an elevated and whitish coloured spot or streak, in some point, near the union of the cornea with the sclerotic coat, accompanied by a greater or less degree of inflammation of the adjacent conjunctiva. The blood vessels in the inflamed part are of a bright scarlet colour, run in a straight direction, and are more numerous towards the diseased spot, some of them being ramified through it. This opaque spot gradually extends across the cornea, acquiring, as it proceeds, an oblong form with a rounded obtuse point. It continues elevated above the natural surface, is distinctly defined, and remains nearly of the same breadth till it reaches the centre of the cornea. If it passes beyond this, it becomes broader, and the breadth increases as it extends, but I have never, in any instance, observed it pass as far as the opposite edge of the cornea. The degree of vascularity also increases in proportion as this spot becomes larger; and if a magnifying glass be used, the whole of the diseased part will be seen crowded with red vessels.

"When the inflammatory symptoms abate, and the progress of the complaint is checked, the vessels distributed on the diseased portion or conjunctiva acquire a purplish colour, gradually diminish both in size and in number, and run in a tortuous direction; the elevation of the membrane subsides, and, if the inflammation has neither been very violent, nor of long duration, its transparency is completely restored. But this is not always the way in which it terminates, as a degree of obscurity sometimes remains after all the inflammatory symptoms and appearances of vascularity have abated. In other instances there is both a slight degree of obscurity, and some red vessels. In one case, where the accompanying inflammation was very violent, the diseased portion of conjunctiva separated completely from the subjacent cornea, came off in the form of an opaque slough, leaving a slight degree of obscurity in that part of the cornea over which it was situated.

"After the eye has been once affected with this disease, it is very liable to subsequent attacks; and although the cornea regains its transparency, and the red vessels disappear, yet every fresh inflammation is more obstinate than the former; a larger portion of the conjunctiva becomes inflamed, and even after the inflammatory symptoms disappear, the conjunctiva remains interwoven with varicose vessels, flaccid and opaque having the appearance of a new formed membrane.

"Most of the cases of this disease which I have had an opportunity of observing, were in children. In one instance, however, it occurred in a lady nineteen years old. I have seen another very remarkable example of this disease in a strong healthy man, about thirty-five years of age, under the care of Mr. Johnson, surgeon at Kircaldy. In this instance it was accompanied with very violent inflammatory symptoms at its commencement; and although the whole of the corneal substance had become cloudy, from the commencement of the disease, yet the patient himself, by examining daily his eye at a glass, gave a very accurate account of the progress of the opacity of the conjunctiva covering the cornea, and described it as beginning at the edge of the cornea, and extending gradually to a little way past its centre. When I saw him, all the inflammatory symptoms had disappeared, the substance of the cornea had regained its transparency, and the affected portion of the corneal conjunctiva remained thickened, of a brownish colour, and interwoven with blood vessels, the trunks of which were formed into a cluster, at the union of the cornea with the sclerotic coat."

This will give a fair idea of Mr. Wardrop's powers of verbal delineation. He afterwards proceeds to describe the inflammation of the other textures of the cornea, and in the subsequent chapters narrates the phenomena of the pterygium of the cornea diseased by fleshy excrescences, by abscesses, by ulcers, by foreign adhering bodies, by ossification, by speck, by staphyloma, by alterations of form, and by effusions of blood between the laminæ of the cornea, or into the interior chamber of the eye-ball.

Mr. Wardrop's book is purely descriptive, consisting of delineations which are too long to quote, and too good to condense; and from the great variety of affections which are treated of in this volume, and the limits to which we are compelled to confine ourselves, we are obliged to refrain from a more elaborate examination than that in which we have already indulged. The volume concludes with plates exhibiting twenty-one different affections of the eye, some of which are executed with remarkable fidelity. That which represents the death of the cornea from lime, is one of the best resemblances we ever saw. The view of the inflammation of the conjunctiva presents the red vessels larger and more distinct than they usually appear in that affection. Ulcers of the cornea do not usually put on that opaque and chalky whiteness which is represented in the plate, although we have seen some such cases. These however, are trifling faults when compared with the many and the great merits both of the plates and the text. For truth and delicacy of colouring the former are at once unprecedented and unrivalled; and will, we doubt not, amply repay the elegant labours of the artist. The young practitioner, who with the common portion of knowledge concerning the diseases of the eye, undertakes to treat the affections of this organ, will if he is an attentive observer, frequently meet with appearances to which both in his experience and reading he is a stranger. For aid and instruction in such cases, it is useful to have upon one's book-shelf clear descriptions and accurate drawings of the various affections of this organ, arranged in a manner convenient for reference. We know no book which for this purpose we can so strongly recommend as the treatise before us.

Mr. Wardrop is a young man, and has not yet attained all the excellence of which he is capable. He is one of those promising plants which deserve culture. He has visited all the great schools of surgery, both English and continental, studied the treatises, seen the operations, and attended the practice of the first oculists of Europe; and by his assiduity as an observer, his knowledge as an anatomist, and his dexterity as an operator on the delicate organ which is the favourite object of his attention, has become the Ware of the Scotch metropolis. Independent of all other obligations, we are much indebted to him for his attempt to extract practical utility from the pathological principles of Bichat. The French physiologist has cut the road, and erected the finger-post, and Mr. Wardrop has undertaken to travel the way directed, with a meritorious assiduity. Bichat is one of those strong minds whose peculiar merits, although unprecisely stated by some and considerably over-rated by others, deserve to be thoroughly known and appreciated. He excels in a particular operation of mind. He delights his readers not by the ingenuity of experiments to ascertain a fact, nor by the fertility of conjecture to explain a phenomenon, but by the minuteness of his distinctions, and by the multiplied, novel, and unexpected reasons by which he defends them.

Darwin excels as an ingenious explainer; Bichat as a minute observer; Darwin by the fertility of his intellect, Bichat by the nicety of his discrimination; the mind of Darwin is a camera which shews an assemblage of remote objects; Bichat's is a microscope which magnifies small ones.

Art. IX. *Researches, Anatomical and Practical, concerning Fever as connected with Inflammation.* By Thomas Beddoes, M. D. 8vo. pp. 256. 6s. London, Longman and Co. 1807.

Dr. Beddoes is already so well known, as not to require that we should say much respecting him on the present occasion. His learning, both in and out of the profession, is acknowledged; and his zeal to promote medical science, has been evinced by the number of his publications in different departments of it. We wish we could add, that his success in these had always corresponded with the benevolence of his intention; but regard to truth constrains us to say, that in his eagerness to promulgate new discoveries, he has frequently published as *facts*, what his subsequent experience convinced him were only ingenious *speculations*, and his candour led him afterwards to avow as such.

Even the present treatise affords a fresh proof of impatient haste, for it was composed in the space of a few weeks after the publication of Dr. Clutterbuck's INQUIRY; and the abruptness of style, and want of orderly arrangement in the materials, make it appear much more like the *adversaria*, or common-place book of a student, than the deliberate work of a profoundly learned and well experienced physician.

Hitherto Dr. B. has appeared chiefly as an original author: he now comes forward in a new character, viz. as a critic upon the work of another; and it is surely creditable to his strict impartiality, when we find him the very first to expose the errors of a treatise dedicated to himself. Indeed this circumstance renders our task somewhat difficult; as it is obvious, that commenting upon a criticism, must be like exhibiting the shadow of a shade, and especially as we have already had occasion to tread over the same ground in our review of the INQUIRY. Dr. B. however, does not confine himself entirely to the critic's office; but has interspersed his remarks with many valuable facts and suggestions from his own store, which we shall take occasion to notice as we proceed.

Dr. B. begins by observing that—

"In an inquisitive age, opinions spring up, flourish, decay, and give place to others, with a rapidity to which we scarcely find any thing equal, except among the processes of nature in tropical regions. Of modern medical opinions, a number have required scarcely more years to go through their destined periods than those of antiquity did centuries. Succeeding authors imputed to spasm—to a contest between the capillary and the larger vessels,—to debility, direct or indirect, an eminent share in the production of fever. But these hypotheses, if I do not mistake their present condition, are so irrecoverably gone, that it remains only with the historian of our art, to find a place where they may be deposited beside their deceased predecessors, and to erect a decent monument to their memory. A knowledge of the electrico-chemical operations of life, or of those processes (by whatever name they may be called) which produce the alterations of substance incessantly going on throughout the animated machine, would doubtless throw light upon all, and particularly upon febrile disorders. Nay, it is probably in vain to think of an arrangement of facts, so copious as to deserve the name of theory, till we have made great advances in such knowledge. But what has hitherto been hazarded upon the change of composition in the fluids and solids by fever, from the author's own conjectures inclusive, down to the recent formidable labours of Dr. Reich, (*Fieber-lehre, in two vast 8vos. Berlin,* 1805) appears alike destitute of useful application and of evidence. P. 1, 2.

"But the proof of fallacy in speculations that run astray merely from pushing too far, need not dishearten us. We may yet hope to gain essentially, by adhering strictly to the comparison of symptoms, not only with one another, and with the effects of medicines, but with the appearances after death. So that without relaxing in zeal, we have only to limit our endeavours by our powers.

"To the appearances after death, certain reasoners on fever seem not at all to have adverted; by the generality they are but cursorily noted, and in the gross. Seldom indeed, have they been sedulously examined, with a view to fix the alterations proper to this form of complaints, to invent methods for detecting such as are not immediately obvious, and to connect the whole as far as our proficiency in morbid anatomy will permit, with the successive conditions of the diseased system.

"Nevertheless, in the multiplicity of medical records, there are actually to be traced vestiges, and more than vestiges, of a doctrine founded upon these considerations combined: partially it appears in the writings of physicians of no very modern date. Nay, such a doctrine, both in particular and in general, has been laid out with all its evidence in form. Among ourselves, it has recently been advanced, as a new and fruitful combination of ideas; the author by strong implication, laying claim to that distinction, so properly held out by the father of medicine, to the physican who shall produce something superior to his fellows on acute disorders, as being those which cut off the majority of mankind. P. 4.

"The idea (*of fever depending on inflammation of the brain or its membranes*) does not, however, appear to lie deep. It is indicated by the disturbance of the sensorial functions; and beyond doubt was early contemplated, but on inquiry rejected. Thus, in the 16th century, Cotterus the able anatomist of Groningen, speaking of brain-fever, declares, *nullam nec in membranis, nec in cerebri substantia deprehendere quivi inflammationem.* Thomas Willis (our countryman) almost a century later, thinks it more probable that the disease should depend on inflammation of the spirits (*spirituum phlogosis*) than of the brain, or its membranes. Our celebrated cotemporary J. P. Frank, is more pointed still: *Nec tamen sibi quisquam a summis furiosisque hinc in deliriis capitisque doloribus hac in febre—nervosa nempe—pro latente in encephalo inflammatione imponi sinat; fallacitatis quam plurimum hic latet.* Lib. c. 1. 109."

"But of those who have clearly conceived, and consistently pursued this opinion, Dr. Marteau de Grandvilliers, of Aumale, in France, is the first on whom it can bring real profit to dwell."—

Here Dr. B. gives an abstract of his memoir from Vandermonde's Journal, for 1758; and adds p. 17, that an epidemic fever at Wetzlaar, in 1794-5, 'found in Dr. Wendelstadt, an 'observer equally decisive in his opinions and practice, and 'more comprehensive in his views. (*Hufeland's Journal, for* '1795.)'

Passages from Bonetus, Lieutaud, Stoll, Tode, Pew, and

Rush, might be quoted in support of the same idea; but as these stand less instructively connected with decisive practice, Dr. B. passes on to the very extensive *Inquiry on the Seat and Nature of Fever*, by Dr. Clutterbuck, in which, both from the motto of "Give to airy nothings, a local habitation, and a "name,"—as well as from the passage that "physicians neither "agree among themselves, as to what fever is, nor in what it "essentially consists; nor have they assigned to it any certain or "determined seat," *(pref.)* 'it is clear, says Dr. B. that the 'author considers himself the first to whom this opinion, in its 'comprehension and fulness of evidence, had occurred.'—Dr. B.'s intimate acquaintance, however, with the modern continental writings, has enabled him to correct this statement in point of fact; having been informed several years ago, that the same doctrine was taught by Dr. Ploucquet, of Tubingen, and at last procuring two academical tracts upon the subject, defended before that professor, and avowedly corrected by him, in 1800 and 1801. From these dissertations Dr. B. has given extracts, with corresponding passages from Dr. Clutterbuck's Inquiry, on *fourteen* different points; from which it appears, he says, that the latter 'contains the same opinion, supported 'by the same proofs as Dr. Ploucquet employs: the coincidence 'of exposition being such, that the English may often seem a 'diffuse translation of the Tubigen professor.' P. 23.

We would observe, however, that although so many instances of parallelism may reasonably excite a suspicion of our countryman being a mere copyist, yet that as far as regards the public, the question of *originality* in the author, is but of secondary importance, compared with that which respects the *truth* of the doctrine itself. We shall, therefore, leave the former point to be settled by those who may think it worth their trouble; and proceed to notice such matters as bear upon the question of verity.

In p. 41, Dr. B. asks—'By what test shall we try the opinion '*that fever depends upon some inflammation within the cranium?*'—'The state of the functions as it results from that of the organs, is to medicine what trigonometry is to the art of surveying. To this we have seen our authors resorting for their rules. Dr. Clutterbuck, (p. 200—17) transcribes at large, and Dr. Ploucquet quotes a passage in which Dr. Home deduces it as a highly probable consequence that—'all low fevers arise from, or are connected with, an obstruction of the brain,'—because, during an indisposition of his own, he felt as if febrile symptoms were connected with inflammation of that organ; as also the dissection of a fever-patient by the same, in which purulent sinuses were detected in the brain, although no symptom of phrenitis had appeared.

'Displaying a case or two so widely may seem, in effect, similar to a contrivance of certain nimble-fingered performers, who engage the eye of the spectator on some prominent object, that they may imperceptibly make the dispositions out of which the surprise is to issue. Most certainly, nothing unfair is intended; and we often enough see the framer or advocate of an hypothesis, practising upon himself as if a spectator (only in perfect good faith) with a dexterity equal to that of the most expert professor of leger-de-main. But I seriously question whether settling with some exactness the value of post-obituary appearances, would not have been a more suitable preliminary than those considerations *on the laws of the system in health,—on the nature of disease,—and on the locality of disease,* which we find at the head of Dr. C.'s ingenious works. One may apprehend that both writers are somewhat too indulgent towards their own doctrine, when after laying peculiar stress upon the evidence from wide-spreading devastation in the organ supposed to be principally concerned, they subtilize fever-producing inflammation into a mere thickening of the arachnoid coat, and then join in commanding all traces of it whatever, to vanish with life. Which seems pretty nearly saying that every sort of alteration indicates a a febrile inflammation, or somewhat akin, while the *absence* of alteration affords no proof to the contrary. A rule deducible from such reasoning would be fairly entitled to enter into a receipt for the composition of an irrefragable hypothesis.'

With such unlimited postulates, indeed, we might admit any one conclusion just as well as any other; and the Aristotelian maxim of *de non apparentibus et de non existentibus, eadem est ratio* would be completely reversed, for we should be required to believe almost in proportion as evidence was wanting; and that such evidence of the point in question is certainly wanting in a great proportion of cases examined, the following extracts abundantly prove.—In a fever at Geneva, in 1805, which destroyed 40 persons out of 150, in some 'a congestion of blood 'was found in the brain, without any particular alteration in 'the other viscera. In some the congestion was inconsiderable; 'and in others the brain was found in its *natural state.*' P. 46,7. —In an acute typhus at Leipsic, in 1799, described by Dr. Eisfield, the marks of inflammation were by far the greatest in the thoracic and abdominal viscera; and the describer says— "the pains in the head were sometimes so violent, and the delirium so furious, as to indicate inflammation of the encephalon, which, however, was discovered in the abdomen."—In another epidemic in Spain, in 1800, Professors Ameller, Sabater, and Ramos found in the abdominal viscera sanious and purulent effusion, with gangrene. It is expressly said, that in the cavities of the head and thorax essential changes were seldom found. " The alterations in the abdominal viscera were " the only ones that can be considered as the direct and imme- " diate product of the essential morbific cause." (Berthe, *Precis de la Maladie d'Andalousie*, 1802,)

' Dr. St. Firth, house-surgeon to the Philadelphia Dispensary, ' who seems to have used the great opportunities of dissection which ' he had, tells us, (in his Dissertation on Malignant Fever, 1804,) ' that the brain was *generally* in a disordered state; but that the sto- ' mach was *always* diseased: great inflammation observable through- ' out the intestines, bladder, lungs, pericardium, and heart.'

' Dr. Mitchell, (*Philad. Med. Mus. p.* 3, 4,) mentions three dis- ' sections after the yellow fever, in which the brain was *not* affected; ' and in other cases when he left the head unopened, he saw much ' disease in other viscera. In persons who had been affected with ' the disorder in an extreme degree, at the Bush-hill Hospital, Dr. ' Cathrall, (on *synochus maligna*, 1794,) found the brain " entirely " in a natural condition," but the stomach and intestines greatly ' diseased. It is strongly in favour of the accuracy of these dissec- ' tions, that they were made by Dr. Physick.' (*Researches p*, 48—56.

' Contrary to what is suggested by Dr. Clutterbuck (p. 91—3,) ' I am led (says Dr. B.) from the account of epizootic maladies, to ' suppose that brutes are in proportion as liable to fever, as to other ' complaints.'

Indeed the febrile nature of the *distemper*, as it is called, among horned cattle, is here supportd by a host of the first authorities, most of whom consider it as a pestilential fever.

" Dr. Camper after describing the symptoms as those of putrid ' fever, (Leçon iv. *sur l'epizootie*) represents the abdominal viscera, ' larynx, and pharynx, as constantly inflamed, and the lungs as ' frequently so. He opened only one head, and found the brain ' quite sound *(fort sain)*; and he says he did not look for any par- ' ticular disorganization there, as the animals retain their senses to ' the last moment.'

Dr. Von Schallern, employed by the Prussian government to investigate its nature in Bayreuth, says the abdominal viscera and the lungs were the parts chiefly disorganized: the brain " he always found in its natural state," *(Anweisung, Bayreuth*, 1797*)*. Dr. Reich the writer on fever, minutely treats of this distemper, and after describing the striking marks of inflammation and gangrene in most other viscera, adds, " The brain in *almost all cases*, I have found perfectly sound and natural."

Similar testimonies are given with respect to the *distemper* occasionally observed among cats.

" The College of Health at Pavia, in the report drawn up by Dr. Brera, (1798,) exhibits a picture if possible, still more exactly resembling typhus. In their numerous and careful dissections, they found the cavities of the *head* and thorax for the most part, unaltered; while the liver shewed black spots, the gall-bladder was distended with very dark bile, and the small intestines greatly inflamed. Dr. Knebel at Gaerlitz, found *but in two* among a *number* of cats dissected by him, the vessels of the head tinged with dark coloured blood." (*Res. p.* 56. 63.)

The extracts we have given, are but a small part of those afforded by Dr. B. who observes that—

'They in concurrence with all that remain, appear to me to suggest the following gross inference, which alone I shall this moment content myself with drawing: that *in idiopathic fever, the stomach and contiguous parts have been found more constantly and more deeply affected with inflammation than the brain and its membranes.*' (*Res. p.* 65.)

We might here perhaps safely close the evidence upon the question, as amply sufficient to decide it; as Dr. B. however has not contented himself with a multitude of *direct* proofs, but has adduced a number of *collateral* ones, against the *phrenitic* theory of fever, we cannot entirely pass them by unnoticed.

'The anatomy of intermittent fevers, frequently shews the head exempt from disorganization. Fizeau, (*Sur les Fievres interm. Paris* 1803), found every thing perfectly sound, or only a little serum in the tissue of the pia mater, three scruples in each lateral ventricle, and about an ounce at the base of the cranium; whereas the *spleen* was *five times* its natural bulk, and otherwise altered. So the other viscera.'

'During a fever at Toulon, in 1761, which often appeared in the same individual as tertian, then double tertian, and lastly continued, and sometimes was what is called, putrid or bilious, or bilious putrid, Dr. La Berthonaye opened three subjects. The brain and chest were found quite sound, (*fort sains, parfaitement sains,*) in all, though the head had been greatly affected. The stomach alone appeared to be the seat of the disease, its side being coated in the first and second, with different laminæ of a bilious effusion, of the consistence of jelly, and of a deep mulberry colour. In the third, the epiploon was nearly mortified, the gall-bladder full of clotted bile, of the colour of theriaca; the pylorus contracted, inflamed, and adherent to the pancreas, which was scirrhous.'

'Dr. Casimir Medicus, in the large collection of his adversaria, (p. 243—70.) relates a case where quotidian fever supervened upon epilepsy. Nothing could exceed the disturbance of the sensorial functions; "the patient died (says the author), a terrible example "of the malignity which may associate itself with intermittent fever." The abdominal viscera, the omentum, stomach, intestines, and liver, were so completely gangrenous, as to render an examination of them impracticable. "But this man had, to my astonishment, "a well preserved brain. I examined it with the most scrupulous "care. Mr. Womrath dissected it minutely in the presence of several surgeons. It would be in vain to say, that the changes of "structure are too minute to be detected. I have excellent eyes, "and am too well acquainted with the (natural) state of the brain, "not to be able to perceive alterations on looking for them carefully." The same result is offered by other dissections of patients whose sensorium has repeatedly suffered from an acute supervening upon a chronic disorder. J. G. St. Andrien became insane, as it was supposed, from the lead cholic. He had a fever which left him still insane. Some months afterwards he was again seized with fever and died. Nothing uncommon in the *brain.* The whole colon was marked with black spots, internally and externally, gall-bladder, pale and having the same spots. Le Cat who made the dissection, had it especially in view to discover whether the brain was affected. When intermittents are not curable by febrifuge medicines, but disappear on the removal of a stricture in the urethra, no body can doubt where the head quarters of the complaint lie. Dr. Frank sen. describes a quartan which resisted all possible drugs for several months. The patient was cutting a *dens sapientiæ.* The gum being lanced the ague was seen no more. Sympathy may be called in here; of that hereafter. But with this aid, it might (not) be difficult to account for partial or local fevers; a part becoming independently cold, hot, and thin, moist with sweat, at regular periods. Having seen such phenomena, I must consider the numerous accidents from them as generally true." P. 82—Here Dr. B. subjoins a case in his own practice.

We were pleased to find, under the head of *Analogical Considerations*, p. 113, that Dr. B. had received the same impression as ourselves, of something like sophistry in the reasoning of Dr. C. He observes, that,

'The medical philosopher, should hold in suspicion every doctrine which requires the support of auxiliary hypotheses; which obliges to shift or explain away the meaning of terms; or stands opposed to clear analogy. Both our authors on fever, I think, attenuate into nothing the import of the word *inflammation*, or require us to allow to an inadmissible extent, the evanescence of all the traces of the process; altogether arbitrarily, presupposing, (*Clutterbuck, p.* 117.) that they shall be supported by the result of future discoveries. Dr. Ploucquet, openly makes it a postulate, that the brain, in the great majority of cases, may be inflamed, as it were *by proxy.* If the same neither is, nor is to be, a postulate, on the part of Dr. Clutterbuck, is it (not) from his assuming fatal inflammation of the head, in the very cases where gangrene of other vital organs, stares the anatomist in the face?'

"Were our means of observation, (says Dr. C. *Inquiry p.* 177) more accurate, and our diligence greater, it is probable that in most, if not all, fatal terminations of fever, (where the patient is cut off by the fever itself, and not by supervening diseases), we should be able to detect some change in the colour, consistence, transparency, or other physical property of the brain, indicating a corresponding change in the action of its vessels." But in p. 174, he had said 'The essential part of this, (viz. fever,) as of most other primary diseases, consists, not in the altered *structure* of parts, but in *perverted actions*: change of structure is a remote effect, a consequence merely of the morbid *action*, and is what may, or may not take place. Hence if the disease prove fatal before such alteration of structure is induced, (which may well be supposed to happen with respect to an organ upon the state of which all the functions of the system more or less immediately depend), few or no traces of the disease can be expected to be seen after death."

If such be the case, then, we would ask, what would avail any *more accurate means of observation*, or *greater diligence*? would they enable us to perceive that which " may *not* take place," and of which " *no* traces can be expected to be seen?" Dr. B. very justly remarks on the latter quotation,

'On meeting with passages like this, the reflecting reader might believe that he misapprehended his author, were there not so much about downright inflammation in the rest of the work, and were the doctrines not evidently suggested by those very rare cases in which ravages from inflammation have actually been found in the head. The *Essay on Human Understanding*, according to Mr. Tooke, is simply an essay on words. The *Inquiry on Fever*, may turn out an attempt towards defining the single word *inflammation*, and labour perchance not ill bestowed either.' *Note p.* 156."

If Dr. Ploucquet, and his follower, (at least in point of time) Dr. Clutterbuck, can persuade themselves that the following cases shew the brain may be in *inflamed by proxy*, they must certainly admit them as proofs, that it can also be *cured by proxy*. 'A girl of 7—another 8 (*years old?*) ate some of 'the morellus furiosus—all the limbs became agitated by weak 'convulsive movements—the gestures were audacious, the look 'expressed fury; immoderate sardonic laughter succeeded bitter 'tears. They stammered out saucy expressions, and tried to 'bite and tear every thing in their way—the sphincters were re'laxed, the inferior extremities palsied;—this state of phrenzy 'lasted 24 hours—an emetic brought up the morell as fresh as 'when it was swallowed. Half an hour after the operation, 'they were at play, and felt no subsequent inconvenience.' (*Researches* p. 142.) A boy between 13 and 14 began suddenly to talk in a very strange and wild way, and at last became quite ungovernable. As no suspicion of poison, rabid bite, or mental shock could be traced, an anodyne, and antispasmodic vermifuge was given with seeming success. But the phrenzy returning again whenever he was placed on his feet, it was discovered on inquiry that four years before, he had wounded his right foot with a piece of glass, and that two days before the attack, he had complained of being pinched by tight shoes. On examining the foot, a small reddish elevation appeared near the ball of the great toe, and when this was pressed, the paroxysm returned with great violence. An incision being made upon it, a very minute portion of glass was extracted; and much as the patient had raved during the operation, with equal suddenness did all the symptons vanish: he was surprised on being told of all the senseless things he had uttered, and begged pardon. (*Hufeland's Journal*, iv. p. 227.

'These facts (adds Dr. B.) and the speedy subsidence of symptomatic fever upon the removal of a diseased limb—as also of epilepsy and of irritations under a different form, upon cutting a partially divided nerve completely through above the injury—or taking out a little humour that presses upon a nerve, are to me probable proofs, that these symptoms all depend upon some more subtle, sudden, and variable movements than are observed in inflammation. I would not say that inflammation may not depend upon a different exertion of the same influence. But certainly in a degree corresponding to the disorder of the sensorial functions, it does not instantly come and go, as I have shown phrenzy to do. I should indeed imagine that this whole cause as far as depends on evidence from delirium, spasm, convulsions, stupor, and the like, might be dismissed in a single sentence or two. *In the bodies of patients who have suffered under the violence of such symptoms for a long time, no sign of past or present inflammation in the head, has on the most careful examination been detected: they are therefore no indications of inflammation there.*" *Ib.* p. 145-6.

Though our extracts have already much exceeded our prescribed bounds, we cannot help inserting one more, as of great practical value, and coming nearer to what has been verified by our own observation and experience, than all the matters previously discussed in either treatise.

'Having shewn (says Dr. B.) by superfluity of evidence, though much might be added, that in violent fever, whether foreign or domestic, whether yellow or of a different hue, we have a right to assume inflammatory disposition in the *abdominal* viscera, I propose the earliest application of leeches to that region. They should be laid on by relays of dozens; and intermediate subtepid fomentation will at once encourage the bleeding, and subdue the fever of the subjacent organs. Some years since I had written a letter to America, containing this proposal; but suppressed

pressed it from a feeling that I could have no pretensions to advise the very enlightened physicians of the United States in a case under their own inspection. I am led to mention the suggestion by my present plan, and I do so in the belief that it will be found useful, although a late respectable writer, who has rather confirmed a revived principle than improved its application, enjoins us to abstain from applying leeches in peritonitis, till by a general bleeding some 'diminution of pain shall have been effected. (*Pemberton on Dis. of Abdom. Viscera,* p. 5.) I have no comparative series of cases treated in both ways, nor does he produce any such. But in the worst possible cases of peritonitis (of which I apprehend all the varieties, may be understood on the principles discussed in the preceding pages), I see this method succeed perfectly. And in many inflammations, drawing blood from the seat of disease so much expedites the cure that I apprehend this should be universally done, and as early as possible; when the affection is deep seated, blood should be drawn from the nearest skin. On applying leeches to the chest, I have noticed the disappearance of suffusion in the eyes. We know that the effect goes deep. It will extend a foot perhaps.'

The remainder of the volume consists of remarks upon some of the more important indications in the treatment of fevers, with a few briefly narrated cases in illustration; and concludes with three *pious wishes,* (PIA VOTA) viz. 1st. The establishment of a school to teach *morbid* anatomy;—2d, The formation of a Medical Pocket Book, or Vade Mecum, on a short aphoristical plan;—and 3d, A collection of original observations on fever, to be called PYRETICUS; for which he offers himself, to arrange and compile such materials as shall be communicated to him with that view. With regard to the first, though certainly desirable, we believe the improvement must come (as most other great ones have) from the zeal, industry, and fortunate situation of individuals. The accomplishment of the second presupposes that of the *third;* and with respect to this, we will venture to predict, that Babel itself did not present greater confusion of *tongues*, than the PYRETICUS would do of *observations, opinions*, and *practice*.

We are now to take leave of Dr. B. and we cannot do so without gratefully acknowledging the instruction we have received from the numerous valuable documents he has laid before us; to many of which it is probable we should never otherwise have had access. Nevertheless, we still adhere to the maxim of *sat cito, si sat bene*, and are convinced that this volume would have fulfilled the author's object much better, had its publication been less hasty, its arrangement more systematic, and its style more uniform and plain. For although to the few who can of themselves extract the gold from the rough ore, it signifies little in what state they receive it, yet to the many, for whose use it is ultimately destined, a previous refinement is indispensably necessary.

ART. X. *The Muscular Motions of the Human Body.* By John Barclay, M. D. F. R. S. E. &c. 8vo. pp. 580. 12s. Edinburgh, Laing; London, Murray, 1808.

EVERY judicious attempt to extend the knowledge of the human structure, must be received with peculiar satisfaction by those who are solicitous for the advancement of medical and chirurgical science. In the treatment of diseases which have their seat in the viscera, many perplexities arise, resolvable only by the most patient application of that assistance which anatomy affords. An intimate acquaintance even with the situation and action of parts merely muscular, is in many instances of the greatest consequence to the physician: his diagnosis may thereby be rendered more correct; and he may be enabled to avoid the pernicious error of treating as muscular or spasmodic, affections of a very different nature and requiring for their removal, means directly opposite to those which must otherwise be employed. Formally to recount the advantages which the surgeon may derive from extensive anatomical information, must now be superfluous; it is universally allowed to be inseparable from the successful performance of the duties comprehended within his professional department.

Such being our opinion of the importance of anatomy, we shall be credited when we say that we entered with no inconsiderable hopes of gratification and instruction, on the perusal of a work from the pen of Dr. Barclay; report having made us favourably acquainted with his character as an assiduous, useful, and intelligent teacher. It will also be anticipated from what has been already expressed, that much of the value which an author is disposed to attach to his favourite pursuit, we were most willing to affix to that of Dr. B. but we were compelled to suppress a high expectation in reference to the enlightening influence of anatomy, which the first part of the introduction was calculated to excite, lest we should with disappointment at last discover, that even by all the learning and industry of Dr. Barclay, it could not be realized. In a second dedication, addressed to his pupils, (the first, being addressed, *sicut mos est*, to Dr. Thomas Thomson) we are presented with the following reflections:

"From the very few elementary substances that enter into the nourishment of plants, regarded as simple, because chemistry has not yet analysed them, are produced all the wonderful varieties of solids and fluids, of magnitudes and forms, of odours, of colours, and of organizations, that are to be found in the vegetable kingdom. By new combinations of the same substances, are likewise produced all the varieties of solids and fluids, of magnitudes and forms, of odours, of colours, and of organizations that distinguish the different species of animals. The mode, however, in which many of these combinations are formed must, from their minuteness, for ever elude the most penetrating eye; and from their intricacy, or from unknown causes concerned, for ever bid defiance to human research. *It is only in the muscular motions of animals* that the wonderful manner in which many of these combinations are formed, may be traced to *causes that are visible, distinct, and within the reach of every observer*, and this happy and illustrative instance of the method in which the Sovereign of the Universe produces an incalculable variety of effects, by the means of a few secondary agents, must render the motions of the animal system highly interesting to every studious philosophic mind."

Of the parts in which it is probable that any of these transforming combinations occur, the author has not afforded the slightest hint: we venture however, to conjecture, that they must take place in the ultimate portions of the arteries, or at the commencement of the veins, or adopting the idea of Dr. Darwin, in the interposed cells, or lastly, in a set of vessels which may be termed the nutrient, proceeding immediately from them. These parts must be so exceedingly minute that we can never hope to ascertain whether they are muscular or not: but should we succeed in establishing their muscularity, we must necessarily remain in ignorance as to the manner in which the acknowledged effect results from their action. It still remains to be doubtful, or at least it is still doubted by many, whether any thing analogous to muscle exists in the vegetable system; and therefore, the prospect of explaining the manner of their support by attention to the muscular actions remains very far from encouraging. All that can be gained, we conceive, is in the one instance a more perfect knowledge of the muscles which co-operate in conveying and spreading the manure; and in the other, of those which collect and prepare food for the mouth, which divide it whilst there, and which afterwards carry it to the termination of the alimentary canal.

If the occasion of an incident, often most distressing and humiliating to the regular practitioner of surgery, be fully accounted for in the following quotation, we have no doubt that by diffusing the knowledge of it we shall essentially contribute to extend the inquiries recommended by our author. Speaking of luxations and fractures, he asserts, that

"Hitherto the surgeon, in these operations has seldom called in, and could seldom call in, the aid of myology; and hence his success, depending so much on force, accident or manual dexterity, he has sometimes been equalled, and with the vulgar at least, been often surpassed, in point of reputation, by the ignorant bonesetter."

That we have not been aware of the inefficiency of the numerous anatomical teachers who have successively appeared, and some of whom we have been accustomed greatly to esteem, we are compelled to acknowledge; nor did we expect that all the industry which we have heard of and observed in dissecting rooms, was altogether unavailing. Certainly we did believe that opportunities for acquiring a knowledge of myology had not only existed, but been profitably employed. Recollecting, however, that many *distinguished bone-setters* have flourished in the vicinity, and triumphed over the acquirements of some of the most eminent surgeons, we cannot ascribe their vaunted superiority to the ignorance of those whom popular folly may have permitted them to supersede.

If however, that information respecting the action of muscles which was sufficiently correct and practical, could no where be obtained prior to the appearance of Dr. Barclay's publication, he ought carefully to have excluded every thing, which by encreasing its price or giving to it a more formidable magnitude, might interfere with its extensive circulation. On this account, we think that the first part, consisting of one hundred and sixty pages, nearly one third of the whole volume, might have been advantageously omitted. Very little use can arise from the display of the regions into which the muscles are arranged, whether according to Albinus, Innes or Dumas. Dr. B. confesses that "the division is arbitrary, and resembles the division of a book into chapters;" it is indeed often nothing more than an enumeration of the muscles, brought together without any such connection as will make them more easily retained in the memory; or occasionally a *region* is announced, and we find it occupied by a single insignificant muscle. Thus we observe the fifteenth region entitled *Sub Pectore*, and one muscle only, the *triangularis sterni* occupying it; the twenty-ninth or *pedis extremi digitorumque ejus dorsum*, includes the *extensor brevis digitorum pedis*; and the fourteenth, *Inter abdomen thoracemque*, also contains one muscle, a very important one it is true, the diaphragm. Within other regions we meet with very large assemblages, such as in the eighth, *collum*, thirty-five; and the nineteenth, *Dorsum cum lumborum posteriore parte et* *cervice*, thirty-three. Several of these regions in order to render the divisions serviceable, ought certainly to be subdivided, yet Dumas, by whom this has been attempted, is accused of "frittering the body into too many parts." If "the only obvious consequences that can follow are an additional burden on the memory, already overloaded, and some more difficulties thrown in the way to retard our progress towards matters of importance," it was surely useless and unnecessary to insert his attempt. The names of the muscles adpoted by Dumas being exhibited in this condemned arrangement, need not have been repeated along with those of Albinus and Innes, (were it even necessary to repeat these as synonyms in alphabetical order,) for, "so far from being any improvement," we are told "that like conundrums, rebuses and riddles, they are fitted only to amuse children, in their leisure hours." The author then expresses his regret that this celebrated physiologist, "influenced by a *freakish whim*, or a passion, should ever be engaged in such puerile conceits." Judging, however, from the indignant tone of Dr. B. we might conclude that the unfortunate *French* nomenclaturist, instead of being guilty only of preparing for the public a few "puerile conceits" in anatomical language and arrangement, had been engaged in designing and executing the most ferocious movements of revolutionary madness: he is represented as talking "with enthusiasm of his improvements, while he is turning light into darkness, and bringing things back, so far as he can, from a certain degree of order and arrangement, to a state of the wildest confusion and anarchy." p.*48.

Of the first part, to the introduction of which we object because we think it will convey very little information, the remaining portion of ninety pages consists of the "synonyms prior to the time of Albinus," extracted from the third book of *Historia Musculorum.* Now this can only be consulted by a few persons who are prosecuting anatomical inquiries for other purposes than their immediate application to practice, and such will possess and consequently may refer to, the original. By those who must depend upon their skill as anatomists for protection against the unmerited ascendancy of ignorant bonesetters, we apprehend, it will seldom be examined.

A tabular view of "the muscles peculiarly belonging to the osseous structure, arranged according to the several bones to which they are attached," is next introdued, formed principally by uniting the plans of Cowper and Winslow, and thereby at once exhibiting the several attachments, whether of origin or insertion. This, which like the former table, for *substantial* reasons no doubt,' is placed very diffusely on the pages, may as well as a similar arrangement in Dr. Hooper's compendium in an English dress, be of some use to the student; although it will generally be found that a concise description is preferred, even for simple reference.

In the second chapter of this part of the treatise, the author enters upon the consideration of the structure and action of muscles. We are informed that they are "composed of carneous and tendinous fibres, interspersed every where with cellular membrane, and the ramifications of arteries, veins, absorbents, and nerves, and *all of them irritable.*" p 218. That irritability is a property of cellular membrane and nerves, we did not previously know, and are not yet quite prepared to admit; and we regret that Dr. B. who is much too precise to make the remark without sufficient authority, should not have put us in possession of the facts by which it is supported. For our *particular* information it is announced, that, "the *carneous fibres* constitute *flesh.*" Of these fibres collected into small faciculi, and these again into faciculi still larger so as to form a muscle, the strength and weakness according to the presence or absence of vitality, are thus delineated:

> "As they derive their principal power from a vital source, the change produced upon them by death is sudden and obvious. Hence the muscles that, when living, could have ruptured their tendons, *lacerated the bones*, or *broken them to pieces*, can scarcely, when dead, if it were not for their tendons, their cellular membrane, and the ramifications of the sanguiferous and absorbent system support their own weight." p. 218.

This statement, in both the circumstances to which it alludes, is we apprehend, an instance of considerable exaggeration. Dislocation which here appears as an ordinary possibility of the exertion of muscular power, seldom if ever occurs by the exertion of that power alone; and with the exception of the patella, not an example of the strongest osseous structure, we believe bones are never broken by the action of the muscles affixed to them. The sentence quoted might indeed lead us to suppose that it was not uncommon for the patella instead of being simply divided into two parts, to be separated by this *enormous* force into twenty. Whilst that state of the quiescent muscle denominated its tone continues, the separation of its particles, as is well known, is resisted much more than when in consequence of death the tone is lost. yet it is somewhat singular, if, at least we do not consider the direction in which the resistance was designed to be employed, that it should remain undiminished in the transverse direction: this, Mr. Carlisle, who very improperly terms the resistance of the muscles the cohesive attraction, has asserted. From many experiments performed in a very unobjectionable manner, he has determined that the living or "irritable limb sustained a weight *one sixth* heavier than the dead limb" of the same animal. [Croonian Lecture, Ph. Trans. Vol. 95.] It will not be doubted that the strength of a muscle, when divested of its tendinous cellular and vascular accompaniments, must be much diminished; but this must also be the case with the living as well as the dead muscle; and until the difference stated is verified by a comparison of the parts under similar mutilation in both circumstances, we cannot admit to its full extent the observation made by Dr. Barclay.

The third chapter entitled "on muscular motion," is chiefly occupied by investigations of a mechanical nature, the author having with great propriety waved the consideration of the physiological part of the subject: we say with great propriety, because we do not now possess those facts which are adequate to the elucidation of this astonishing and mysterious function of the vital fabric, and because we do not think the talents of Dr. Barclay adapted to the removal of scientific obscurity. The application of muscles as moving powers, to the three kinds of levers described in elementary works on mechanics, is exhibited by suitable figures. "Every lever" we are told, "is *a sort of balance;* and the parts of the balance extending from the fulcrum or centre of motion to the points on which the power and resistance operate, are respectively the levers of the power and resistance." p. 278. Now a lever is only a sort of balance, when it is employed statically and not for overcoming a resistance; and a balance which he has chosen for illustrating the lever, in these same elementary works he would have found defined to be a lever with equal arms, used for comparing the quantity of gravitating matter in different bodies, and therefore having no lever of power and resistance. Dr. B. has likewise committed the common mistake of supposing, that by the adoption of a lever of the third kind "there must be a waste or expenditure of power, as the muscle in consequence of a lever, must exert a force by many times greater than the resistance which it has to overcome." p. 279 Some writers on mechanics have undoubtedly sanctioned this false conclusion, but if Dr. B. had examined several of "the writings of those who have treated on *the nature of Dynamics,*" he must have discovered that although the resistance moved when a weight is visibly less than the power, yet there is such a compensation in velocity as to render the momentum or effect not less advantageous than in the first and second species of lever. The account which is introduced of the action of oblique muscles and the fibres of muscles obliquely placed, generally correct, is derived from Borelli and Monro: the physiological paradox advanced by the latter is however avoided. Dr. Monro has asserted that by dividing four muscular ropes five inches in length, into parts one inch and a quarter long, and then placing them in such a position as to obliquity, that *the whole* would *lose two-fifths* of its force, the power would be *greatly augmented*. For, "as there are *sixteen* fibres instead of *four*, their force will be as sixteen multiplied by three, to five multiplied by four, or as 48 to 20, or as *twelve* to *five*." [Edin. Phil. Trans, vol. 3.] Had the all-discovering professor paused in his distinguished career, to consult the 121st propositition of the Treatise de Motu Animalium, a work which he has mentioned, he could not thus have offended the spirits of Cocker and Borelli.

In the last part of this treatise, containing "different arrangements of the muscles, according to the motions in which they co-operate," we encounter most frequently that peculiar phraseology, which in another work published by Dr. Barclay for the purpose of explaining and recommending it, is called "A new Anatomical Nomenclature." This appears to us a misnomer, for words previously used are alone employed, with terminations altered to adopt them to application, as adjectives or adverbs: the change, if we may be allowed to construct a word to designate it, we would term *telology*. Whilst examining the cacophonous language which Dr. B has produced, we were occasionally reminded that he might, undesignedly, have suggested one or more of his own impulses in the following paragraph; "At this time of day, when no real inconveniency is felt, any alteration could only proceed from misguided ignorance or a species of mania for nomenclaturing." [New Anatomical Nomenclature, p. 109.] The observations which we intended to have made on this language and its application, want of room obliges us to omit; and we cannot now do more than add, as a specimen of it, and of the mode of describing the direction of the action of muscles, the following extract from sect. 2, on the position and motions of the scapula.

"In man, where the clavicles, with the intermediate bone of the sternum, greatly exceed that diameter of the trunk where they are situated, the two scapulæ are removed from the lateral aspects to the dorsal, and with their heads pointing atlantad, laterad, mesiad, and sternad, and their bases sacrad, mesiad, and dorsad, admit of a certain change of situation, not only in directions atlantal and sacral, but ster-mesilateral and dorsa-mesial.

"The motions of the scapula are, Atlantad, Sacrad, Ster-mesilaterad, Dorsa-mesiad, Rotatory, or combinations of these motions.

"The muscles by which these motions are performed are the Trapezius, Rhomboidei, Levator scapulæ, Serratus anticus, Serratus magnus, and the directions of their action At. dor. mesiad, At. ster. laterad, Sac. ster. mesiad, Sac. ster. laterad; assisted ocsasionally, through the medium of the humerus, by the Latissimus dorsi. Pectoralis, (in the directions) Sac. dor. mesiad, At ster. mesiad, Sac. ster mesiad, and through the medium of the clavicle, by the subclavius, (in the direction) sac. ster. mesiad.

Justice to the author demands that we should express our opinion, founded on a very attentive perusal of his performance, that it will afford considerable improvement and assistance to those who are disposed minutely to trace the separate and combined actions of muscles. We sincerely wish that the patience and industry displayed in its composition, which we cannot too much applaud, may more generally prevail amongst those who devote themselves to inquiries connected with the science of medicine.

ART. XI.—*A Popular View of Vaccine Inoculation; with the Practical Mode of conducting it: shewing the Analogy between the Small Pox and Cow Pox, and the advantages of the latter.* By Joseph Adams, M. D. F. L. S. Physician to the Small Pox and Inoculation Hospitals, &c. 12mo. p.p. 161. 4s. London, Phillips, 1807.

BUT a little while ago, we looked forward with complacency to a period at no great distance, when that ravager of beauty and destroyer of life, the small pox, would become extinct, for want of subjects capable of propagating its poison. Sorry are we to find, that this event is now more remote in prospect than before; and still more to declare our opinion, that this retardment is as much owing to the *professed friends*, as to the open and avowed enemies of vaccine inoculation.

With a liberality which equally characterizes true science, and a benevolent mind, Dr. Jenner, as soon as he had substantiated by experiment, the practical and general application of that fact which had been locally notorious for generations, stept forward and gave the result of his observations; throwing himself upon the candour of his brethren and the public, for their verification or disproof. The leading fact was so novel, as to be at first received with much distrust; and we confess ourselves to have been among the number of those, who withheld their assent till borne down by the prodigious daily accumulating mass of evidence.

The difference between the external appearance of the two diseases, and the absence of any general eruption in vacciola, were such striking circumstances, that they seem to have entirely divested its adoptors of even prudent distrust, with respect to the latter, and led them into a hasty belief, that what ever they saw produced different from small pox, must necessarily be the *genuine* cow pock; and this point once decided, that the patient was, *ipso facto*, ever after secure against the former.

The number of subsequent failures, however, which, (as might be anticipated), increased with the spread of vaccination, have unhappily proved the error of such a conclusion; and while they have weakened the faith of the well meaning, have afforded the selfish and malignant few, an opportunity to raise afresh the war whoop of *down with the cow pock!*

The press has literally groaned for some time under the wordy labours of the opposing parties; and it is a doubt with us, which have *in fact* done most injury to the cause of truth and humanity, the enemies or the *friends* of vaccination. By the former, we are threatened with hideous deformity, loathsome disorders, and almost extinction of the human race, as the consequences of its general adoption; while, by the latter, we are flattered not only with great improvement in human beauty and health, but with the prevention of many destructive maladies now existing; and have even been gravely told, that various fatal distempers incident to the brute creation, as the *pestis gangrenosa* in cattle, the rot in sheep, and the hydrophobia in dogs, &c. &c. would ultimately be done away by vaccination: there wanted but one step farther to crown this ascending climax of absurdity; that man thus freed from a host of physical evils, would have his age protracted to that of the patriarchs, or even live for ever!!!

Heartily tired of this contention between selfish ignorance, and benevolent enthusiasm, we gladly hailed the appearance of a short treatise, that *promised* to discuss the matter in a plain and candid way, and set the general question of preference entirely at rest. Such we hoped this Popular View would prove; as the author might not only be supposed to come to the task well prepared by the nature of his former labours, (on morbid poisons), but superiorly qualified by the opportunities which his present situation must afford him, of comparing the two diseases under all their possible varieties. How far it has answered our expectation in this, will soon appear.

The author begins by stating, (p. 1.) that,

'Extraordinary as it may seem, we have no certain accounts of 'the first introdction of small pox. This has induced some to believe 'that it existed from the remotest periods. But we have in scripture, '*most accurate* descriptions of those diseases which were deemed un'clean or contagious, and none of them applicable to the small pox.'

After remarking on the silence, and thence deduced unacquaintance of the ancients, respecting one uniform and notorious character of small pox, viz. its taking place but once during life,—which *must* have been known to them if the disease had occurred then; and the strong impression that our knowledge of this fact has made upon us with regard to other infectious febrile diseases, (as the plague), he concludes it to be—

'Highly probable, that the first introduction of this contagion, 'was by the crusades. Ambrose Parey, (Paré), assures us, that 'after that event, the leprosy became so frequent, that not less than 'twenty-one thousand lazarettos were erected in France. It is difficult 'to conceive, (continues Dr. A) that these could be for any other pur'pose, than for the reception of those afflicted with so general and so 'contagious a distemper as the smallpox. This is the more probable, 'as it was soon found that in populous towns it was scarcely possible 'to avoid the disease, and that such as had once passed through it 'were no longer in danger, many of these lazarettos fell into dis'use, till at length the introduction of inoculation rendered them 'no longer objects of general attention.'

That the sacred writers, who, from necessity joined the medical character to that of the priesthood, and practised medicine merely as an empirical art, should nevertheless be competent to describe contagious diseases with '*great accuracy;*' whilst Paré and his contemporaries, whose education was systematic, and whose sole attention was given to medicine and surgery as a science, should confound a *novel* and acute disease, with a chronic complaint, which had been long familiar to them, and was already so *accurately* described in the Bible, is an opinion too repugnant to common understanding to be admitted upon bare assertion. We therefore turned to Paré's works, and though not without some doubt as to what we might find there, yet certainly not with any idea that the evidence would have been so complete. Our readers, then, will judge what was our surprize, on finding that small pox, measles, and leprosy, were not only described, each under its own *name*, in such a way as clearly to discriminate them from one another, but that, while the two first as being allied in certain circumstances, were considered in one chapter, (the 1st of the 20th book), the last, viz. leprosy, was put in a separate chap. (the 6th), 6th), and identified with the Elephantiasis. As to the twenty-one thousand lazarettos said to have been erected for the leprosy, (or as Dr. Adams will have it, for the small pox), though we will not assert that it is not contained in Paré's works, yet as our utmost diligence has not been successful in discovering the least trace of such a passage, the statement of it must, for the present, stand upon even less ground than the well-known legend of St. Ursula, and her *eleven thousand* virgin nuns, who were said to have cut off their noses to escape violation from a barbarous soldiery; for the critic, (a heretic no doubt,) who translated *Undecimilla* as a *proper name* instead of *eleven thousand*, still admitted that there might have been *two* such heroic virgins; whereas we cannot find even *one* lazaretto.

Though it may be said that this has little to do with the main object before us, yet we consider it of great consequence, that on so important a point, and where much may turn upon the *reputed judgment* of an author, he should, upon examination, be found strictly correct in every one of his positions; and it surely cannot bias us in his favour, to discover that he has thus made a palpabable mistatement at his very outsetting.

The second chapter is entitled '*On the causes to which we may attribute the advantages of receiving the small pox by inoculation.*'

'With whatever disease the human body is seized, the effect, like 'impressions on the mind, is much increased by the suddenness of the 'shock.'

'Sydenham first remarked, that the most favourable small pox 'usually makes its appearance on the third, sometimes on the fourth 'day of the fever; whereas, in the most violent, the eruption always 'appears about the second, or at most the third day. If the pustules 'in the face, ('where they first appear'), are numerous and confluent, 'the patient will be in the utmost danger, however distinct and even 'sparingly, they may appear on other parts of the body.'

'From all this, which future observation has confirmed, it follows 'that the danger depends on the *force* and *extent* of the *first shock*. 'That is, if the shock is violent, this will appear by an *early* 'eruption of pustules, attended with inflammation proportionate 'to the actions excited. If therefore, the effect of the first shock 'could be limited in its extent of surface, it follows, that the danger would be *proportionally* limited.

'Such is the *object* of inoculation; and in proportion as we succeed 'in it, we secure our patient from danger. By introducing contagious matter at one particular part, we confine the first shock to 'that part. Hence the advantage of always using *fresh* matter, which 'will be *likely* to produce a more certain and quick effect on the part.

'It must be still more important to use the *clear* fluid taken from 'the *eruption* in its *early* stage, because at that time *only*, it affords 'the *genuine* infection. When it afterwards becomes thick, it is 'mixed with matter formed for the purpose of healing the pustule; 'and sometimes if taken in its later stage, it will consist of nothing 'else, and disappoint the inoculators altogether.'

On these extracts we shall remark, that the first passage contains a position, which, generally speaking, is correct; but although the reasoning upon it, applied to explain the advantages of inoculation, is at least as probable as any other, yet it is by no means free from its difficulties; for if an *early* eruption of pustules be a proof of a violent shock, and of course proportional danger in the casual disease, how comes it, that the inoculated small pox which outruns casual infection, and consequently must have an *earlier eruption*, should yet be so much milder*? and upon the same principle too, how is it that (as Dimsdale has remarked), the sooner the infection shews itself active in the arm, which must bring on the general infection and eruption more speedily, the more favourable the disease will be? It appears to us, that the difference generally observable between the disease as excited in one and the other mode, depends upon the first impression being made in the casual mode, *directly* upon the brain through the olfactory nerves, while in the inoculated it is communicated *indirectly* through the medium of distant branches. This is corroborated by the comparative ill success of inoculation in China, where it is performed by thrusting up the nostrils, dossils of cotton impregnated with variolous matter; and by the violence of the symptoms in a young female convict, upon whom (Dr. Mead tells us) the experiment of inoculation was made in this way. In experiments made for the express purpose, multiplying the number of punctures, and, *a fortiori*, increasing the *extent* of the first shock, has never been found to augment the violence of the consequent disease. One would imagine from Dr. A's. statement (p. 13,) that the *first variolous impression* in the casual disease, was made upon the *whole face*, and in the inoculated, upon a mere spot on the arm, whence it was communicated to the whole system; but this is not correct, for in whatever way the disease be induced, the face is the part in which the pustules first appear after the general commotion has begun.

To those who may adopt Dr. A.'s reasoning, the *object* of inoculation will, no doubt, appear what he alleges; but the multitude who were inoculators, had no other *object* than that which experience taught them, the fact that inoculation did produce a milder disease, without their either knowing or enquiring *why* or *how* it did so. Just such is the present amount of our knowledge respecting vaccination—viz. that it is milder than small pox, and a preservative from it, and, *therefore*, should be preferred.

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* 'The inoculated small pox is earlier by at least four days in producing the eruptive fever.' *Dr. Adams*, p. 57.

As to the kind of matter employed, though it be unquestionable, that the variolous fluid has the full power of infecting while yet in a limpid state, yet the testimony of thousands equally go to prove, that it does not, like the vaccine virus, lose either its efficacy or its mildness by becoming puriform; nay that the hard scab macerated in water, was equally successful as the recent fluid. The permanency of the variolous infection is strikingly opposed to the fugitive property of the vaccine; and is no slight argument against their identity, which is alleged by Dr. A. The bramins of India, we are told, innoculate with matter handed down for two or three generations, *(Vide Essâi sur l'Inoculation, par M. Chais)* the infection has been known to spread through a village, upon opening a grave in which a person dying of small pox had been interred *twenty years* before, (*See the account in a book called the City Remembrancer,)* and the late Dr. Woodville found that after nearly boiling away a pint of water from a drachm of variolous pus, for three successive times, the residuum still communicated the disease by inoculation. We would not, however, assert, that a choice in the matter is absolutely indifferent, and, therefore, to be neglected; for although multiplied trials have shewn, that a mild disease has been produced by matter taken from a confluent pock, yet where there is an alternative, we would certainly prefer that taken from a benign kind, and from a healthy constitution, were it only to preclude all possibility of accusation on this account, either from others or from ourselves, should the disease prove unfavourable.

The third chapter, *On the various kinds of small pox, and on the approximation of the most favourable to the cow pox,* announces a *discovery*, and founded upon that, a *practical improvement*, which if confirmed by future observation, will entitle Dr. Adams to solid proofs of public gratitude, and secure to him a niche in the temple of fame, scarcely inferior to what Dr. Jenner has obtained; for there are many who have still so much doubt about the preservative power of cow-pox, and others so influenced by their fears of its consequences, as to make the alternative of small pox highly acceptable, could it be rendered to a *certainty* mild.

Dr. A. says,—'Though during an epidemic small pox, we see the disease in a variety of forms, sometimes even in different individuals of the same family, yet there are certain distinctions which Sydenham first made, and others which have been noticed since his time. That great master first marked the difference between the confluent and distinct small pox; and also in the appearance of the disease at different seasons. Though subsequent writers have made other divisions, yet there is one *kind* which I do not recollect having seen described by any author. Formerly when the disease was more common, the kind I allude to, used by the nurses to be called a *white sort*. Such, I conceive, was the small pox mentioned by Sydenham, which he says left no pock marks; different from what happened in a subsequent part of the season, when even the distinct sort was followed by pock marks in the face.'

'Dr. Jenner, in his "Inquiries into the nature of the cow pox," speaks of a particularly mild sort, which spread for some time over Gloucestershire. Sydenham makes no other distinction between his two kinds of distinct small pox, than that the mildest left no pock marks; and Dr. Jenner gives no other description of his, than that they never became confluent; that a number of cases proved as mild as the inoculated disease, and that the lower classes of people so entirely lost all terrors of it, that the usual intercourse was maintained as if no such contagion existed. I suspect from its universal mildness, that this must have been the *white sort*, the most striking peculiarity of which is, that, contrary to what Sydenham observes, both of the confluent and distinct, the pustules remain white on the face, as well as on the rest of the body, till they begin to scab.' p. 21—4.

This *white sort* Dr. A. describes as 'never very large, but round, and uniform in proportion as the disease is well marked, the upper surface extending over the base, and drying into a scab nearly globular, of a pale amber colour, and much harder than in the common distinct disease. From the figure, colour, and other properties preserved throughout the whole progress, I shall call this kind of small pox the *pearl sort*. The nearer the resemblance is preserved to a pearl, and the smaller the individual pustules, the more perfect is the character. p. 24—5.'

'It might be suspected, as this variety of appearances in small pox, seems to depend on the degree of inflammation, that it could make no difference with what kind of matter we inoculate, but that the degree of inflammation and subsequent disease, would depend on the state of the person inoculated. It is very certain, that when small pox inoculation succeeds as it usually does, the kind of matter is unimportant, because the inflammation at the arm secures the constitution from any subsequent shock, and, with proper attention, the general eruption is not of sufficient consequence to excite alarm. But it is well known, that this is not *universally* the case, it is, therefore, desirable to *secure* a favourable eruption, and it seems confirmed by experiments? that there is more uniformity between the matter inoculated and the disease produced, than has hitherto been supposed. By continuing with great caution to inoculate at the hospital from pearl small pox, and afterwards by selecting those arms which had most the appearance of cow pox, we at last succeeded in procuring a succession of arms so nearly resembling the vaccine, that an universal suspicion prevailed among the parents that they were deceived by the substitution of one for the other.'

Here follows a register of cases at the Inoculation Hospital, in support of the alleged amelioration in the quality of the variolous matter. The first patient was inoculated from the casual small pox; the *vesicle* of a *vaccine appearance*, and 150 pustules.—Second case, inoculated from the first, *vesicle vaccine*, *no pustules*.—Third case the same.—Fourth case, *arm vaccine throughout*, 150 pustules.—Fifth case, *arm vaccine*, about 500 pustules, dried early.—Sixth case, *arm vaccine*, 60 pustules.—Seventh case, *arm perfectly vaccine*, a few pustules. Of seven others inoculated from this, five had the *perfect vaccine arm throughout*, but *no eruption*; a sixth, *irregular vaccine* arm, *no pustules*, and the seventh *perfect vaccine arm*, with 100 pustules, appearing on the 12th day, and drying on the 16th. Of four others inoculated from these, *two* had *regular*, and one *irregular vaccine arm*, but no pustules; and *one*, small pustules, drying early. From a patient (the 6th case) who had pustules, two were inoculated from the *arm*, and two from the *pustules*; the two from the *arm*, had 'the *legitimate vaccine* appearance:' one from the *pustules* had fever with general *efflorescence*, (no pustules); 'the other had *all the symptoms of vaccination*; but the contents of the vesicle became *yellow* before it dried; a circumstance which Dr. A. remarks, (p. 40—1) makes the only unequivocal distinction between the *two* diseases.'

Let us now examine the force of these arguments *seriatim*, and see to what they amount. That in some seasons the small pox proves very mild, and in others extremely severe, is a fact which must be within the personal experience of every man who had been several years in practice before vaccination was known; and that this should have been remarked by Sydenham and Dr. Jenner, is not surprising. But what does this prove? Surely nothing more than what we see with regard to other febrile contagions, as measles, scarlatina, &c. that under the influence of certain causes, at present unknown, but most probably atmospheric, the same specific contagion will produce very different effects. For if, as Dr. A. would have us believe, the *kind* of the disease in small pox, depends upon the *quality* of the matter, from what source was the particularly *mild* kind mentioned by Sydenham and Jenner produced? Was it from a similar mild kind which they took no notice of, or was it from that which possessed no such particular mildness? Again, what became of this *stock* of mild infection; did it die away, and leave only the more virulent breed to perpetuate its ravages, or did it lie dormant, and only shew itself at such distant intervals as to excite the particular notice of practitioners from its being unusual? But farther, why does Dr. A. rest so important a point upon mere *suspicion*, when, by applying to Dr. Jenner, whose candour is equal to his discernment, he might have added *his powerful testimony* to his own *suspicion*, and thus have given it a greater weight, if not in his own estimation, at least, in that of others? This plan of breeding a milder small pox, somewhat resembles the late Mr. Bakewell's plan of improving the breed of sheep, by selecting the most perfect animal of each sex, and successively taking the most perfect of their offspring he at last produced a race of animals possessing a greater number of perfections than any that had before existed; but Mr. Bakewell's improved sheep still remain; Does Dr. A.'s improved small pox still continue? or must we, to perpetuate its benefits, still recur to its original source, the Small Pox Hospital, *where alone* the alleged requisites of fresh limpid matter, taken from the pustules of one patient and inserted into the arm of the other, can possibly be procured, and (on the score of courtesy, at least, if not of justice and policy) to the *original improver*, who superintends that hospital? For no private practitioner in the present day, or even when small pox inoculation only was in use, could pretend to have such a constant succession of patients, as to supply him with recent limpid infection at all times, and thereby to render their patients *universally* secure of a mild disease.

With respect to the register of cases given by Dr. A. after the most impartial examination of the evidence it contains, we are obliged to say, that we think it amply justifies the suspicions of the parents, that the disease communicated to their children was *not* genuine small pox, but a *variolated vaccine infection*, exactly corresponding with what repeatedly occurred to the late Dr. Woodville, when he began the vaccine inoculation at the small pox hospital; for let it be noted, that of the twenty two cases registered by Dr. Adams, scarcely one but had the *vaccine* *arm*, *fourteen* had *no* secondary pustules, and of the *eight* who had, most of them dried away very soon; just as we find to be the case, where a patient was exposed to the variolous infection at the time when the vaccine influence was *in progress*, and before it had arrived at that ultimate stage which gives complete protection to the system.*

CHAP. IV. *Containing presumptive proofs, deduced from the Laws of all other Morbid Poisons, that the Variolous and Vaccine is the same.*

'It is a law in *all* local as well as constitutional diseases, that no two of them will continue at the same time, in the same place, or the same constitution. In local diseases this can hardly be questioned, because it would be *absurd* to say that two different effects could be produced at the same time on the same individual spot. But in constitutional diseases it might be supposed, that two of them might produce their respective actions at the same time, in two different parts of the same body. The contrary has, however, been proved in so many instances, that it is now *almost* universally admitted. The small pox in *every* instance where it has interfered with the measles, is found to be arrested in its progress till the latter has gone through its periods, after which the small pox has recommenced its actions, and required for its completion the full number of days, without reckoning any of those in which it was interrupted by the measles.'

A similar power, Dr. A. says, belongs to chicken pox in suspending small pox; and in both chicken pox and measles in suspending cow pox; whence 'it follows, that the action of measles, and also of chicken pox, is different from the action of small pox or cow pox, and that they cannot be maintained at the same time in the same constitution.'

'But if small pox and cow pox are inserted at the same time in different parts of the same person, we find no interruption whatever in the progress of either. Both begin and go through the usual courses with the same regularity as if only one of them had been inserted in two different places.' p. 41.

These circumstances Dr. A considers as amounting to a 'proof of the identity of the two morbid poisons.' P. 41.—He admits, however, as objections to his conclusion, that the small pox is *generally* attended with secondary pustules, (p. 42); that small pox may be communicated by *effluvia*, but cow-pox only by the *application* of the virus (p. 43.);—and that small-pox is always accompanied by fever, while cow-pox has but little or none, (p. 72.) But to these he replies, that small-pox has occurred without eruption, and that though secondary cow-pox pustules are rare, they have occurred too often to be questioned:—that the effluvial mode of communication in small-pox, only 'shows a higher degree of contagion in *two* diseases (!) *both* of which are contagious;—and that is by no means certain, that they are not different *degrees* of contagion, in different *kinds* of small-pox:'—lastly, that in small-pox the degree of fever is uncertain, and oftentimes as mild as in the severest cow pox. P. 45.

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* Nay, Dr. A. has admitted, that small pox pustules may be excited even on the skin of those who have had small pox, merely by the skin being fretted while the person is exposed to a variolous atmosphere (p. 89). But we would readily refer this point to an *experimentum crucis*, that of inoculating with a *mixture* of variolous and vaccine matter; when we will venture to predict, that the result will exactly agree with the appearances here described.

The suspensive power of some diseases over others, has long been noticed as a *fact*; but was first considered as a *law of disease* by the late Mr. Hunter, who, like his disciple and admirer Dr. A. held it to be *universal*. But though it has doubtless *often* happened, yet the exceptions to it are sufficiently numerous to prove it not universally true. Etmuller mentions a case (upon which however, we would not lay much stress on account of its singularity) where the 'measles occupied one side of the body and the small pox the other.' (Op. t. ii. p. 1. c. 10.) Two cases of their *concurrence* will be found in the Ed. Med. Com. vol iii; and a third may be seen in one of the early numbers of the Medical and Physical Journal by a Mr. Rolfe, a practitioner at Bristol; and a longer search than our time affords, would no doubt discover many others. Desessarts relates 'a complication of small pox with scalatina (*Journ. de Med.* t. xlix.); and we have unquestionable authority to say, that measles supervening upon hooping cough of a fortnight's standing, greatly *aggravated*, instead of suspending the paroxysms.—These testimonies are surely sufficient to disprove the alleged universality of the law; nor is it at all difficult to explain, why such exceptions should not oftener occur, for it requires, that the *weaker* specific action should have advanced so far, that the *stronger*, though it may just *overtake*, cannot *outrun* and *suppress* its influence; whence they both appear in full force at one particular point of time.

In Chap. v. Dr. Adams endeavours to shew *the probability that the first improvers of small pox inoculation owed part of their success* to the insertion of a favourable kind of small pox.

'From the two preceding chapters (he says) it appears that 'small pox has certain varieties, and yet these varieties appear '*sometimes* permanent when communicated by inoculation to 'other subjects, and yet this and other circumstances (already 'noticed) have led him to suspect, that the Suttons were partly 'indebted to chance for their *early* success; i. e. that accident in 'the first instance, directed them to the choice of a pearl small pox, 'which was afterwards for a long time perpetuated by the '*manner* in which inoculation was communicated to new 'subjects at their establishment. For besides that the practice 'has certainly never been in other hands so uniformly success'ful, as described by *impartial* people under the Suttons, there 'is *incontestible evidence* that *their own fame* declined; and 'this we may presume (to have been) from the failure of that 'success to which they first owed it.'—Here he quotes a passage from a letter written by a clergyman to Sir G. Baker, stating that, "in ten years practice, our operator (Dr. Sutton) has "not lost one patient;" and afterwards inserts an admission by the same correspondent, of two patients having died in the subsequent year, one "a hard drinker, who had been cured of "an ague which he had 5 or 6 months; the other a lady who "had wilfully neglected every direction, and among other irre"gularities, had taken large quantities of spirit of hartshorn "during the disease." (Sir G. Baker's Inquiry.) 'It is not my 'intention, (Dr. A. subjoins) to doubt the truth of the causes 'here assigned; but if these two incidents of irregularity oc'curred in the course of one year, it is very improbable that 'none equally flagrant should have happened in the course of 'the ten preceding years. Besides which, the conduct of the 'lady, however improper, would not be sufficient to kill any 'patient under a *favourable* inoculated small pox.'

That the *fame* of the Suttons declined; the decline of their *fortune*, and their final poverty, 'are incontestible evidence;' but that their rise at *first*, and their downfall at *last*, were not owing to the causes Dr. Adams *presumes* upon, we can shew by *evidence equally incontestible*. An eminent surgeon in London who was domesticated with them for years, assured a friend of ours, that at the period of their greatest splendor, and (presumably), greatest success, they employed matter taken indiscriminately from the *recent* and the *mature*, from the *confluent* and the *distinct* pock; and this corresponds with what Dr. Frewen first affirmed, and what multiplied experience afterwards proved, viz. that the *quality* of the matter was of little or no consequence. The 'decline of their fame,' then, was not owing to any failure in *their success*; but to the success of *others*, who adopted their plan of treatment under inoculation, being equally great; for it is not to be supposed, that people would come themselves, or send their children, from all parts of the kingdom, at great expense and inconvenience, to the inoculating house of Daniel Sutton, when they saw almost uniform success under the management of their own medical friends at home: and, upon the same principle we will venture to say, if even Dr. Jenner himself had declared, that he alone could give security by vaccine inoculation, or if Dr. Adams should assert, that *he only* can do the same with small pox, their respective 'deline in fame' would not be less certain than that of the Suttons.

With regard to the *uniform success* of the Suttons, for ten years, testified by the *venal pen* (as Dr. Woodville indignantly calls it), of the Rev. Mr. Houlton, (Sir G. Baker's correspondent), it is indeed 'very improbable;' yet we think that the lady's avowed neglect of every direction, and other (no doubt *private)* irregularities, together with the large use of the spirits of hartshorn, were quite sufficient to kill any patient, even under the *true pearl kind* of small pox infection!

The sixth chapter is, *On the mode of conducting small pox inoculation*; in which we observe nothing either new or peculiar, except that the summer is the most proper season, instead of the winter, as we had generally been taught to believe:—

> "That the matter should be *fresh*, that is, the subject from whom it is taken should be present:—that diluting the matter if dry, very much lessens the certainty of success;'—but that 'when dry, the best mode of dissolving it, will be by the *saliva* of the patient to be inoculated.'

Upon each of these particulars, very pointed questions might be put; but as these questions will readily occur to our readers, we shall leave them unnoticed; as well as the seventh chapter, which treats of the '*prognostic symptoms of the disease.*'

Having now got through *seven* chapters out of *eleven*, and seventy pages out of one hundred and thirty-one of this *popular view of vaccination*, and finding them entirely taken up in detailing the history of the rise,—the amelioration of the *kind*,—and the improvements in the mode of conducting inoculation for—————————the SMALL POX!!! for that very disease which it is the grand object, and will prove the immortal glory of vaccination to extirpate from the face of the earth!!! we come at last to chap. viii, *The advantages of vaccination over small pox inoculation,'* whence we are told, that,

> "It is no small satisfaction to turn our attention from a disease which is never unaccompanied with anxiety to the practitioner, the patient, and his friends, towards a discovery which promises to deliver us from every doubt, in a case which often requires prompt decision. Such is the cow pox. Without symptomatic fever, (*see p. 313 of our review*), without secondary eruptions, (p. ib.) or without

out danger from *either* (!), and also without danger to others, we secure ourselves, our children, or friends. No season need be preferred, no age is improper, and no state of health (!) has been found prohibitory.'

After disavowing all unfair partiality for cow-pox inoculation he says, 'the first and most important of all inquiries is, Does cow pox introduce dangerous disorders into the human body? I can truly say I have met with none, not even a sore arm, from vaccination. It must be admitted, however, (he adds) that there have been a *a few real cases*, among the many false ones which have exsited alarm; and I shall be glad if by this opportunity I can set the minds of many respectable people at ease, on a question which has produced much unnecessary uneasiness, and not a little ill blood.'

CHAP. ix. *Candid examination of the objections to vaccination.*

'When an unfortunate event from cow pox, has happened to *more than one* person in *a family*, it has been imputed (*by the friends of the vaccine disease no doubt?!*) to some impurity in the blood, and not to vaccination. Nothing can be more cruel than the use of so undefined an expressson; nor can we wonder if such as are thus calumniated, are provoked to retort a language which shows so little delicacy in those who use it. When I mention impurities of the blood, it is to use a word the most general in its acceptation, and consequently the least objectionable. The evil or *scrofula* are terms still more disgusting, but they are all equally undefined, and all equally terrible to the imagination.'

'It is much to be lamented that such expressions should ever have been admitted and highly disgraceful that they are still retained in medicine. As, however, we shall presently see that the subject is not (!) connected with our present purpose, I shall dismiss it with expressing a wish, that those who use such terms, would at least tell us what they mean. All that can be required of us, is, that when we meet the question fairly; and as it cannot be disputed that in a few instances *very serious* complaints have followed the *insertion of cow pox matter*, that we reconcile the admission of these facts with our recommendation of them.'

Into what a labyrinth of contradictions has the author here plunged himself? In one page proclaiming the absence of *fever*, of *eruptions*, and of *all danger* from cow pox; in another telling us the *fever* of small pox is often as mild as that of *severe* cow pox; here assuring us there are *no eruptions*, and there admiting them to have 'occurred too often to be questioned;' now asserting that no danger ensues from either, and immediately after granting that 'very serious complaints have followed the insertion of cow pox matter'!!!

Not less astonishing is his language about impurities of blood, and the equally undefined terms of *Evil* or *scrophula*! Does Dr. Adams mean to say, that there are really no vitiated states of the circulating mass, or secreted fluids, which will aggravate the operation of certain morbid poisons? And is scrophula that undefined thing, which exists but in disgusting name and terrible idea, and which never affords the fruitful soil, whence spring unsightly scars, defective sight, and fatal phthisis? We really want words to express our surprise at such language!!! But let us see how he gets rid of these *terrible imaginary evils*, and reconciles his opinions with those stubborn things called *facts*. He *attempts* to do it, by obligingly *informing* us, that, 'there are certain peculiarities in the *bodily constitution*, as well as in the *tempers* of individuals, and oftentimes of more than one in the same family, which are particularly seen in the effects of certain substances upon the organs of sense connected with it: and instances these in the effects which such persons experience from eating muscles, honey, or almonds; from the smell of cheese; the sight of a cat; the stimulus of a blister plaster; or the bite of a leech! Nay (strange to say) he adds after all this, that these are no more remarkable nor surprising, than that some families have found small pox invariably severe, and often fatal, even under inoculation; that to others again measles, and to others ulcerous sore throat, have proved equally alarming, or even fatal: yet that 'nothing of this kind shews any depravity 'or vitiation in the form or juices of such subjects or family; 'but only a susceptibility (*a most unfortunate one truly*) to be 'affected by these particular subjects in a manner *different* from 'other people!!!'

We have heard of *law* quibblers, and of a criminal being acquitted, though his *identity* was clearly proved, as well as his transgression, merely because he was indicted under a *wrong name*. Whether this will apply to that deeply-accused death-doer, SCROPHULA, or not, we must leave to an impartial jury of our brethren, to whom we willingly refer the question for adjudgement:—to us it is of little consequence whether the mischief arise from a thing called by a Latin or an English name, whether Scrophula,—foul humours,— or only a *susceptibility*; since it is admitted by Dr. A. himself, that there is a *somewhat* attached to individuals and families, which renders them the unfortunate victims of particular disorders when once excited by their specific causes, whether that be the contagion of syphilis, of scarlatina, of measles, of small pox, or of cow pox. The only thing that concerns the public, is, which of the two last ought most to be dreaded? and upon this question which (as that of small pox inoculation *was*) must be decided by length of time, *we* have no hesitation in saying—Small Pox a thousand to one.

The remainder of this treatise consists of the usual instructions for conducting vaccine inoculation, in which we observe nothing new;—it is closed by an appendix containing the queries respecting vaccination, put by the College of Physicians to Dr. A. in common with other medical men of the metropolis, and his answers to these; and it is illustrated with several coloured plates of vaccine and small pox pustules.

We are here to take our leave of Dr. A. which we cannot do without expressing regret, that he should have published any thing which could have drawn from us so much animadversion, as in our opinion tending indirectly to perpetuate the existence of that very disorder, which, it is the general interest of mankind should be entirely done away,—we mean the Small Pox.

MEDICAL INTELLIGENCE,

Consisting of brief Communications of Varieties in Disease, and Novelties in Practice is requested to be addressed to the Editors, at Messrs. Longman and Co. Paternoster Row.

The following Surgical Cases, among others of considerable interest, have occurred in London since our last.

1st. An Aneurism of the Carotid Artery, in a porter æt. 50, in which the Artery was divided. One Ligature separated on the twenty-first, and the other on the twenty-third day. The patient was relieved by the operation in this, as in a former case, from a violent pain in the same side of the Head.

2nd. Aneurism of the external Iliac Artery, in a Gardener, æt. 39, in which the vessel was also divided. The Ligatures came away, one on the fifteenth, and the other on the sixteenth day. Both Patients are still under cure.

An Operation for Carotid Aneurism, has also lately been performed at Stockholm, by M. Burckhardt. The event we have not yet learned. There have thus occurred three Cases of Carotid Aneurism in which the operation has been performed. In some cases of Popliteal Aneurism, the single Ligature has been used with perfect success. The wound required for this practice is much smaller, and the operation altogether more simple. Whether it is equally secure as a general practice remains to be determined.

In October, Mr. Watt, of Paisley, will publish "Cases of Diabetes with Observations." The practice is new, and in many respects the reverse of that which has been generally followed of late years.——The work will also contain "Cases allied to Diabetes," in which, a similar treatment has been successful.

We have to regret the necessity of postponing to our next number, the review of Mr. John Bell's Work on Tumours, and Mr. Wishart's Translation of Professor Scarpa's Treatise on Aneurism.

LIST OF MEDICAL PUBLICATIONS.

Anniversary Oration, delivered March 8, 1808, before the Medical Society of London, on the general Structure and Physiology of Plants, compared with those of Animals, and the mutual convertibility of their Original Elements. By John Mason Good, F. R. S. Senior Secretary to the Medical Society. 8vo. 2s.

Observations on Lithotomy, being a Re-publication of Dr. James Douglas's Appendix to his History of the Lateral operation for the Stone, and of the Original Papers, relative to Mr. Cheselden's Invention and Improvement of that Operation. To which is added, a Proposal for a New Manner of Cutting for the Stone. By John Thomson, M. D. one of the Surgeons of the Royal Infirmary; Professor of Surgery to the Royal College of Surgeons; also Regius Professor of Military Surgery in the University of Edinburgh. 8vo. 4s.

A Treatise on the Anatomy, Pathology, and Surgical Treatment of Aneurism. Translated from the Italian of Antonio Scarpa, With Notes. By John Henry Wishart. 8vo. 15s.

Essays on the Morbid Anatomy of the Human Eye. By James Wardrop, Royal 8vo. 1l. 1s.

Vaccine Scourge; in Answer to the Calumnies and Falsehoods lately circulated by that extraordinary Surgeon, Mr. Birch. 8vo. 1s.

The Vaccine Phantasmagoria. 4to. 2s.

Important Researches, upon the Existence, Nature, and Communication of Venereal Infection in Pregnant Women, new-born Infants and Nurses. By the late P. A. O. Mahon.

A Tract on the Nutriferous System in Men, Quadrupeds, and Birds, and in all Creatures having Livers. By James Rymer, Surgeon, R. N. 2s. 6d.

An Account of the Diseases most incident to Children. To which is added, an Essay on Nursing, with a particular View to Infants brought up by Hand. By the late George Armstrong, M. D. a new Edition, with many additional Notes. By A. P. Buchan, M. D. of the Royal College of Physicians, London. 7s.

An Exposition of the Practice of Affusing Cold Water on the surface of the Body, as a Remedy for the Cure of Fever; to which are added Remarks on the Effects of Cold Drink, and of Gestation in the open Air in certain conditions of the Disease by Robert Jackson, M. D. 8vo. 10s. 6d.

Observations on the Fifth Report of the Commissioners of Military Inquiry; and more particularly on those parts of it which relate to the Surgeon General. By Thomas Keates, Esq. Surgeon General to His Majesty's Forces. 4to. 12s.

The Chirurgical Candidate; or Reflections on Education, indispensable to complete Naval, Military, and other Surgeons. By Charles Dunne, Member of the Royal College of Surgeons, London.

The Edinburgh Medical and Surgical Journal, exhibiting a concise View of the latest and most important Discoveries in Medicine, Surgery, and Pharmacy. No. 15. 3s.

The Medical Remembrancer, or Pharmaceutical Vade Mecum, intended as a complete Pocket Manual. By Thomas Churchill, 12mo. 2s. 6d.

THE

LONDON

MEDICAL REVIEW.

OCTOBER, 1808.

N° IV.

Art. I. *Recherches Physiologiques sur la Vie et la Mort; Par Xav. Bichat, Médecin de l'Hôtel-Dieu, Professeur d'Anatomie, de Physiologie et de Médecine, Membre de plusieurs Sociétés savantes. Seconde Edition.* A Paris, Chez Brosson, Libraire, rue Pierre-Sarrazin; Gabon et Compagnie, Libraires, près l'Ecole de Médecine, An. X. 1802.

WE will not deny that the interest we have derived from a perusal of the works of this extraordinary young man, has created in us a strong desire to determine the justness of his claim to a reputation which has already attained no common celebrity. Considering, however the necessarily limited circulation of his works in this country, and the systematic arrangement which he has universally adopted, we think it proper to present our readers with an impartial analysis of his principal performances, before proceeding to exercise the arduous duty of critical jurisdiction. The check upon our supplies is perhaps one reason why the contributions of foreigners rank so highly in the esteem of our medical literati; for in reputation as well as in finance, *magnum vectigal est parsimonia.* The same argument may be used in apology for the abridgment of their works which are in our possession, and indeed there are few *French* writers who would not be great gainers by the process. If we except those occupied in the abstruser departments of science, there are few or none who do not discover their weakness in an attempt to display their power. It is as impossible for a Frenchman to write a compendious book as a short letter. But in his ostentation to exhibit his acquirements to the best advantage, he forgets, or appears to forget, how much of them is already known. He is reduced to the painful alternative of taking without acknowledgment what belongs to others, or of suppressing what he knows himself. It would flatter him it is true, to be esteemed candid, but it would *kill* him to be accounted dull. Ingenuity however is the fort of the French writers. Few of them commit open burglaries, but most might be convicted of petty larceny. They excel, if we may so express it, in a kind of chemistry of composition; an art of metamorphosing what is recent, and vamping up what is old. Hence the surfeit of their compliments, their grimaces, and their books. Insignificant distinctions, fantastical refinements, frivolous novelties, and eternal repetitions, which would be insupportable but for a certain flippancy of style, are proofs incontestible that it is more agreeable to their natures to *dabble* than to *dip.*

A French system stripped of its gewgaws, though it might appear homely in Paris, has greatly improved its chance of popularity in London. There is in general a severity of style in our scientific writers that steadily maintains the dignity of their purpose; but the antic gestures of a Frenchman's conversation are visible in every page of his book. All general remarks have peculiar exceptions, and we should be equally averse to have these interpreted as expressing our estimation of many old, and some modern writers, as to admit the conclusion, that they are in any degree addressed to the partizans and admirers of Bichat. But we are ready to declare our unequivocal preference of the older to the modern continental authors. They may be less brilliant, but they are more sound. They have less pretension, but more philosophy.

With reference to the merits of the writer whom we have probably the pleasure to introduce to many of our readers, we shall only state, that we are not of the tribe of enthusiasts who look for the coming of a genius (heaven born!) to dispel as by a magic touch the clouds which obscure physiology. We have an inherent dread of *geniuses* as well as *prophets.* To such *we* are all apathy and dullness, and if we were to indulge prepossessions, they would only serve to make us more so. Nature may be taken by siege, but not by surprise. Her mysteries are never surrendered at discretion: they are invested by barriers and bulwarks, sturdy and jealous of access, and such as have yielded much to a vigilant and enduring warfare, but little to the most brilliant sally. Of a young author we augur cautiously, and it perhaps constitutes his strongest recommendation to favour, that he has written with reserve. Of a young and very voluminous author we portend fearfully, and are ready to apprehend that some allowance must be made for quantity, in appreciating the quality of his labours. But this is not the kind of ambition that we would wish to see encouraged. It aims at the *multa* rather than the *multum*. Such a spirit has not animated the sober and strictly inductive inquiries of Hewson and Goodwyn, of Kite and Coleman, of Macdonald and Jones.

But we will not suffer ourselves to entertain a particular prejudice upon general grounds. Far be it from us to insinuate that *much* may not be done *well* even by a young man. We hope by the example of Bichat to establish the injustice of such an insinuation. To the analysis of his Researches we have prefixed a short but interesting description of himself and his studies, which will prove the most natural index to his works.

Marie Francois Xavier Bichat, the son of a physician of the faculty of Montpellier, was born on the 11th November 1771, at Thoirette, in the department of Ain. He was initiated in the study of anatomy under Petit, chief surgeon of the Hotel Dieu at Lyons, and after the siege of that city betook himself to Paris, where he became a pupil of the celebrated Desault. A vigorous application to the study of anatomy and surgery soon acquired for him distinction among his contemporaries, and recommended him to the notice of this distinguished teacher, by whom he was received as the inmate of his house and the partner of his labours. In the year 1797, he entered upon the duties of a teacher, and had recourse to experiments upon animals with a view to confirm the phisiological facts already advanced, and determine with accuracy the point from which to set out upon the discovery of new ones. In this year he established his theory of synovial membranes, a prelude to his treatise on the subject of membranes in general. At the conclusion of his course of anatomical demonstrations, he acquitted himself with singular address in a series of surgical operations. The frequent habit of speaking in public induced at this period an hæmoptysis, which though it created considerable alarm for his safety, arrested only for a short interval the progress of his labours. The succeeding winter found him engaged in a more extended course of anatomy, and the superintendance of a dissecting theatre, which was attended by eighty students. During the same season he augmented considerably the number of his experiments, and payed the last tribute of respectful gratitude to the memory of his illustrious master, by an edition of the surgical works of Desault.*

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* Œuvres Chirurgicales on Exposé de la Doctrine. et de la Pratique de P. J. D sault, &c. &c. A Paris, An. IX. 1801.

His views in phisiology hitherto limited to the facts already illustrated, were at this time principally directed to the system of membranes. The idea of classifying these textures was furnished by the reflexions of M. Pinel, in his *Nosographie Philosophique;* and his first publication on this subject was presented in two memoirs to the medical society of emulation. To these were shortly added three papers upon practical subjects. The first described an improved construction of the trephine; the second exposed the impossibility of luxation in the fracture of the humeral extremity of the clavicle; and the third suggested a new method of applying the ligature to polypi. In a concluding memoir he broached his first doctrine in physiology. This was the division of life into animal and organic. Shortly after the publication of these memoirs appeared his Treatise on Membranes*, which was regarded as an original and classical work. At the same period he commenced regular courses of physiology, into which he introduced the opinions peculiarly his own; and in 1799, illustrated them in a manner more authentic and extensive in his '*Recherches Physiologiques sur la Vie et la Mort.*'

The *Anatomie Generale*, a system of anatomical physiology, describing the minute organization of tissues, as well as illustrating the principles contained in his former treatises, and presenting the further application of them to the various branches of the science, was his third and great work†. To this properly succeeded the *Anatomie Descriptive*, or a description of the whole parts and organs of which the body is composed, of which work he lived to finish two volumes and arrange in part the materials for a third.

From an intimate examination of healthy structures, Bichat passed to the investigation of their changes under disease. In the space of a few months we are informed that he examined upwards of six hundred morbid bodies; now pursuing with as much eagerness the path of Morgagni, as he had before traced the footsteps of Haller. His contributions to this department of science will be found dispersed throughout his works. A new arrangement of the Materia Medica, which was to consist in a classification of its articles according to their influence and operation on the vital functions, occupied the last moments of Bichat. For observation on this interesting subject his appointment as one of the physicians to the Hotel Dieu afforded him an ample field. Frequent attacks of indisposition had effected no moderation of his professional ardour, and he continued to indulge himself in the unrestricted pursuit of his anatomical inquiries even in the warmest season of the year. By such excessive and incessant activity of mind and a total inattention to the means of preserving health, his constitution not naturally strong was ill prepared to resist the effect of accidental violence. A fall upon a stair case of the hospital produced a severe concussion of the brain, which rendered him for several minutes insensible. To this succeeded pain in the head and fever, and he died on the fourteenth day from the accident, viz. 3d. June, 1802, æt. 31. The whole school of medicine mourned for him, and more than five hundred students followed his remains in funeral procession to the grave. By an edict of the consulate on the fourteenth of the same month, it was decreed, "that a monument should be erected in the hall of the Hotel Dieu, to transmit to posterity the names of DESAULT and BICHAT." Valuable moral qualities and engaging manners were combined in the character of this extraordinary young man. Frankness, candor, easy concession of opinion, equanimity of mind and generosity of disposition, were his distinguishing characteristics. To these might be added a manly contempt for the attacks of jealousy, and a heart susceptible of firm and disinterested friendship.

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* The *Traite des Membranes*, is to be regarded as a mere sketch or projet of the plan, of which the Anatomie Generale presents the full and perfect developement, under the titles of cellular, mucous, serous, fibrous, and synovial systems. We have the authority of Bichat himself to consider it in this point of view, and indeed as containing many inexact ideas and imperfect statements. These, in his later work received correction, and the details of structure and function are there considerably amplified. Our readers will therefore agree with us in thinking it an useless addition to our mutual labours, to enter upon a distinct investigation of the merits of this Treatise.

† The analysis of this work will appear in our next number.

The Researches consist, as may be inferred from the title, of two parts, of which the first carries its interest more immediately to the metaphysical, the second to the anatomical physiologist.

Part I. RESEARCHES CONCERNING LIFE.—*Life,* (says Bichat,) *is the union of those functions which resist dissolution.* Living organized bodies are surrounded by agents of destruction. Inorganic bodies act upon them incessantly, and a continual mutual action is set up between them. Were it not for a permanent principle of reaction, death would ensue; this is the principle of life, known only by its phenomena. The habitual alternation from the action of bodies exterior and re-action of the living body, constitutes the most universal of these phenomena. The modification of this alternating action by the different stages of existence leads to the conclusion, that the quantum of life is as the difference which exists between the efforts of the exterior power and the interior resistance.

DIVISION OF LIFE INTO ANIMAL AND ORGANIC.—Viewed in detail, life presents two remarkable modifications; one belongs in common to animals and vegetables, the other to animals alone. The vegetable sustains towards exterior bodies only the relation of nutrition, which constitutes interior life; the animal exists by virtue of its numerous relations with surrounding bodies, and enjoys therefore allied to the interior, an exterior life. Hence the animal possesses two distinct classes of functions. By the first, it exists within itself, by the second, beyond itself. By the first it transforms into its proper substance the molecules of surrounding bodies, and rejects them when they are become heterogeneous. By the second it sees and perceives, reflects, moves, and communicates. By the former class it is fixed to the soil whence it imbibes existence, by the latter it is rendered an inhabitant of the world. The assemblage of the first class of functions constitutes ie *organic*, and that of the second the *animal* life.

SUBDIVISION OF ANIMAL LIFE INTO TWO ORDERS OF UNCTIONS. The first order is that which communicates the npressions of the senses to the brain; the second, that which ommunicates the impressions of the brain to the organs of oco-motion, voice, &c. The first comprehends the action of exterior bodies upon the animal, or *sensation;* the second, the reaction of the animal upon them, or *volition*.

SUBDIVISION OF ORGANIC LIFE INTO TWO ORDERS OF FUNCTIONS. The first, that of composition, resulting from digestion, circulation, respiration, and nutrition. The second, that of decomposition, resulting from absorption, circulation, exhalation, and secretion. The first constitutes assimilation and increment; the second, disassimilation and decrement. Thus the sanguineous system is the centre of the organic, as the cerebral of the animal life.

CHARACTERISTIC DIFFERENCES, PHYSICAL AND MORAL, OF THE TWO LIVES. 1st. The correspondence and symmetry of exterior form in the animal, and its irregularity in the organic life. All the organs of sense and voluntary motion furnish proof of the first, and all the visceral structures of the second position; hence the structures of animal life being, as it were, double, the functions are mutually independent, and may exist on one side while on the other they are suspended; but the converse of this maintains in organic life. 2d. Harmony is the attribute of the animal actions, while discordance is the character of the organic. 3d. Animal actions are subject to periodical intermissions; organic are incessant, and do not admit of continued interruption. 4th. The phenomena of animal life are influenced by habit, as perception and judgment; those of organic life are independent of it. 5th. *Intelligence* belongs exclusively to the department of animal life, and the *passions*, although they have a modifying influence over the animal actions, have their seat in the organic.

DIFFERENCES OF THE TWO LIVES IN RELATION TO THE VITAL POWERS. The vital differ from the physical forces in being variable and exposed to innumerable fluctuations; hence the incalculability of the former, and the misapplication of mathematical terms and calculations. The vital properties are those of function in contradistinction to the properties of structure, and are reducible to *sensation* and *motion*.

There are two kinds of sensibility. The first, or *organic*, is the property of receiving an impression; as the stomach from food, and the heart from blood: the second, or *animal* sensibility, is the property of receiving and propagating the impression to a common centre; as the eye and ear convey light and sound. The organic sensibility is common to animal and vegetable, the animal is peculiar to the former. The nature of these two species of sensibility is the same; the difference is determined by the degree of intensity, the one being, as it were, a maximum of the other. In proof of their identity we may observe, that in many parts the two faculties are connected with, and insensibly succeed each other. Of this, the origin of all the mucous membranes is an example, as in the passage of food from the mouth to the stomach, of urine, &c. &c. the animal imperceptibly changes into the organic, and vice versâ. 2d. Different excitants applied to the same organ, are capable of producing both modes of sensibility. A ligament punctured, or irritated by acids, gives no pain; but a twisting, distension, or laceration of the ligaments, is acutely painful. The walls of an artery do not transmit the impression of its proper blood, but if injected with a fluid strange to the œconomy, the animal instantly expresses pain. 3d. By habitual impression the animal passes into the organic sensibility, as the urethra becomes callous to the introduction of an instrument; and vice versâ, the organic sensibility of a part inflamed approaches by a daily increase to the standard of animal sensibility. Every part endowed with sensibility has its fixed and determinate measure, although subject to continual varieties of elevation and depression. This appropriate sum of sensibility fixes the nature of the relation which exists between different organs in contact with each other. The ordinary sensibility of the urethra is not in relation with the seminal fluid. The lacteals reject all fluids mixed with the chyle, their sensibility not placing them in the same relation with these fluids. That it is the quantity, and not the nature of the sensibility which determines the difference of these relations, is proved by the absorption of dropsical accumulations, and the resolution of tumours. To the elevation of sensibility in the capillary vessels is to be ascribed the passage of red globules through serous tubes, which characterises inflammation; for in the natural state the transparent vessels are inferior in sensibility to the red; and it is only when the former from increased action mount to the same level with the latter, that the relation is established between the transparent channel and the red blood. Without this change of sensibility the globules could not pass, even though the diameter of the white was as triple to that of the coloured vessels. Hence the surfaces most exposed to those agents which heighten the sensibility, as the conjunctiva and cells of the lungs, are most subject to local inflammations; and by the reduction of sensibility to its natural standard, resolution is effected. Why are the organs of the interior life endowed with a sum of sensibility so inferior to those of the exterior? because their agency is placed beyond the reach of volition and consciousness.

There are two kinds of contractility. The first, or *organic*, has no reference to a centre, but is confined to the organ in which it exists, and performs the functions of digestion, circulation, &c. The second, or *animal*, is derived from the centre of its system, has its seat in the voluntary movers, and ceases to exist when the nervous connection between them and the brain is destroyed. Each mode of sensibility is succeeded by its corresponding contractility as a natural consequence, but as the brain and nerves form the medium of communication between the animal sensibility and contractility, they admit of separation; that is, the impression made upon a voluntary mover may not communicate motion, which interruption cannot happen between the sensibility and contractility of organic life. Animal contractility is uniform and indivisible, organic is divisible into two species. The first may be termed *sensible organic contractility:* it is acted upon by considerable masses of matter, as in the heart, stomach, and bladder. The second is the *insensible organic contractility;* and is stimulated by moleculæ, or divided masses, as in the absorbent and secretory organs. The first may be compared to the physical attraction of aggregation; the second to chemical affinity; no diversity of principle exists between them. Like the organic and animal sensibility, the differences of organic contractility are not in nature, but in quantity.

Extensibility and *contractility* are the properties of tissue. These phenomena likewise succeed and maintain a mutual dependance upon each other. They are passive, produced by mechanical influence, and distinguishable from the vital properties in surviving the body until the decomposition of its parts.

Illustration of the various species of contractility. When a voluntary muscle enters into action by the influence of the brain and nerves, it exemplifies *animal contractility:* when by the excitement of a physical or chemical agent, *sensible organic contractility,* or *irritability:* when by the impulse of its circulating fluids, *insensible organic contractility,* or *tonicity:* when by a transverse section of its body, *contractility of tissue,* or *contractility from failure of extension.* The first and second of these are movements of totality; the third, partial and imperceptible oscillations, as of secretion, absorption, &c. Each species may be suspended *per se.* By dividing the corresponding nerve we destroy its animal contractility; by injecting the vessels with a solution of opium, its irritability; by obstructing the passage of blood, its tonicity; and lastly, by putrefaction, the contractility of tissue. The insensible organic, and the contractility of tissue are inherent in every organ. The animal and the sensible organic contractility are exclusively peculiar. Insensible organic contractility may be considered as the general character of all *living* parts, and contractility of tissue as the attribute of all parts, living or dead, which possess organic arrangement.

THE ORIGIN AND DEVELOPEMENT OF ANIMAL LIFE.—The faculties of sensation and perception are almost null in the fœtus. Loco-motion is a sympathetic affection of the voluntary movers through the medium of the brain, such as happens in sleep during digestion, and hence belongs to the department of animal life. The functions of this life are developed by education, and matured by habit. The influence of society upon this education of the organs of animal life, gives a predominance to one sense or faculty, at the expence of the remainder. A fundamental law in the distribution of the vital forces is, that when they increase in one part they diminish in another. The actual sum is never augmented, but transported successively from part to part. This law pertains to both classes of lives. Particular periods of existence are destined to the perfection of particular organs in both divisions of animal life.

ORIGIN AND DEVELOPEMENT OF ORGANIC LIFE. This life commences at the instant of conception. The functions which form a circle of composition and decomposition exist, but the former are less numerous, and the latter almost inactive. Hence, as well as from the non-existence of animal life which admits of the concentration of the vital forces, the rapidity of growth in the fœtus. In the infant, the law of education necessary to animal life is totally reversed. The functions of organic life enjoy instantly after birth, the highest degree of perfection. Hence no one function is susceptible of acquiring a superiority over the others, as in animal life, and such a superiority when it exists, is the result of primitive organization. Thus the physical temperament and moral character are also insusceptible of change, being beyond the reach of education, which modifies so remarkably the actions of animal life. The *character* is the physiognomy of the passions; the *temperament* is that of the internal functions, both of which have their seat in organic life. Education may moderate the influence of the first by maturing the judgment, and so fortifying the animal life as to resist the impulses of the organic, but the attempt to alter the character would be analogous to that of permanently diminishing the number and force of the heart's ordinary contractions.

THE NATURAL TERMINATION OF THE TWO LIVES. The functions of animal life gradually fail, and cease before those of the organic. Social intercourse influences materially the difference which exists between the periods of cessation of the two lives, and the shorter duration of the animal functions may be owing to the comparative excess in which we use them. It is the privation of these which constitutes the fear of death. The sudden destruction of organic life occupies two periods. The first is marked by the instantaneous cessation of respiration and circulation; the second by the slow and gradual exhaustion of irritability in the secondary agents. The natural termination of this life, as in old age, is gradual throughout the several organs of the body. The heart is the *ultimum moriens*. Hence the difference in these cases is, that in the former, death proceeds from the centre to the circumference, and in the latter, its influence commences at the circumference, and finishes in the centre of the system.

Part II. RESEARCHES CONCERNING DEATH. In all sudden deaths organic survives animal life a certain greater or less time; the contrary cannot happen, though the death of both may in some cases be nearly synchronous. The difference of termination depends on the nature of the connection which subsists between the two lives, formed by the brain on the part of the animal, and the heart and lungs on that of the organic. The actions of one organ of this triple centre, (which compose the natural and vital actions of older physiologists,) is indispensible to the existence of the others*.

Division of the subject. 1st. Of the sudden deaths which have their origin in the heart, in the lungs, in the brain. 2d. Of the cause of death in the two surviving organs, from the inaction of one. 3d. Of the mechanism by which the death of the body follows that of the affected organ. 4th. Of the nature of the different maladies affecting the heart, lungs, and brain.

INFLUENCE OF THE DEATH OF THE HEART ON THE BRAIN. The heart must act on the brain through the medium of nerves, or of blood vessels. By the first it cannot, for the nervous (loco-motive) influence never *ascends.* The muscles are paralysed by a ligature on their nerves, but no effect is produced on the cerebrum. Arm a lumbar nerve and the muscles of the superior extremity, and their communication produces scarcely a sensible motion; transpose the second armature from the *upper* to the *lower* extremity, and violent convulsive motions ensue; but tye or compress the cardiac nerves, and the effect is null upon the brain. Blood-vessels, then, are the agents of the heart's influence upon the brain.

The arterial blood may excite the brain 1st. By its mechanical impulse. 2nd. By the nature of its constituent principles. The first is essential to the existence of the cerebral functions. Tie the carotids, and the alternate movements (elevation and depression) of the brain are enfeebled; intercept the whole mass of blood by tying also the vertebrals, and they are annihilated. In both instances the vital energy of the animal suffers a corresponding depression. The effect of blood or pus effused, or bone depressed, is to prevent the organ from obeying its accustomed impulse. Hence the functions of mind and motion are intercepted; remove the compression, and its powers return, a sufficient proof that the organ is *oppressed, not disorganised.* In some cold-blooded animals, the frog for example, the heart propagates no motion to the brain; hence the brain may be totally deprived of blood, and its functions not immediately cease. That the cerebral motion may be too impetuous as well as too feeble, and from either cause injurious, is seen, 1st. by injecting a dog's carotid with water, which, if impelled with moderate force, does not affect him; if violently, it deranges the motion of the brain, producing instant and violent agitation. 2d. By the effect of sudden depletion; for as the hemorrhage increases, the cerebral motion diminishes; therefore, the interruption of action in the left side of the heart produces a similar interruption of action in the brain. But motion is not the exclusive medium of the heart's influence on the brain.

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* Every sudden death is owing to the interruption of one or other of these functions *primarily*, the two functions indirectly affected ceasing successively.

An effusion of blood from the right side of the heart operates in the same manner; and the compression of the jugulars, so as to prevent the reflux of blood to the right auricle, produces, from the counter compression of the stagnant venous and the influent arterial blood, atony and death of the brain. Death produced by air injected into the veins has its origin, not (as many suppose,) in the heart, but in the brain. For, 1st. the heart beats long after the signs of animal life are extinct. 2d. The same phenomena, (excepting the agitation of the heart by the mechanical contact of a foreign body,) follow the injection of air by the carotid. 3d. Air bubbles are found in the left side of the heart, and in the arteries, and consequently are contained in the carotids and vertebrals. 4th. If air be injected into the vena portæ, a much longer time elapses before the animal is affected, as the capillary circulation of the liver prevents its arrival so soon at the brain. 5th. If the brain ceased to act from privation of the heart's action, death would supervene; as in syncope from hemorrhage, i. e. *by default of motion*; but its movements on the contrary are violently convulsive, and announce the presence of an irritant. Hence it is fair to conclude, that in the accidental admixture of air with the venous blood, *the death of the heart is the effect, and not the cause, of the death of the brain.*

INFLUENCE OF THE DEATH OF THE HEART ON THE LUNGS. The mechanical and chemical functions of the lungs are mutually depending on each other. By stagnation of blood in the right ventricle the chemical phenomena are stopped; consequently the brain is not nourished by arterial blood, if it receives any; and the intercostals and diaphragm, losing their nervous energy, terminate the mechanical actions. When the left side is obstructed or wounded, the brain receives a powerful impulse, its motion is accelerated, and consequently its action on the muscles, the contractions of the diaphragm and intercostals, and the mechanical functions of the lungs are all increased. As in the former case, the chemical phenomena were impeded for want of blood, here they are stopped for want of air; for if the right side of the heart receives the injury, death begins in the chemical, and terminates in the mechanical functions; if the left, the order of cessation is reversed.

INFLUENCE OF THE DEATH OF THE HEART ON THE OTHER ORGANS OF THE BODY. The brain rendered immoveable is inert: the animal actions (sensation, loco-motion, and voice,) cease. Cold-blooded animals, whose brains receive no motion, are an exception. To the motions also of organic life, which are partial and oscillatory, as in the liver, spleen, stomach, &c. and not of totality like those of the brain, the arterial blood is necessary; for being withdrawn, the intestine motion of each particular organ and the general agitation produced by the excitement of the surrounding parts depending on the brain, immediately cease. Thus the heart exerts on the other organs two modes of influence. 1st. Direct. 2d. Indirect: viz. through the medium of the brain. The functions of nutrition and those depending on capillary circulation, being in some measure independent of the centre, are gradually suspended from deficiency of materials. A proof of the excitation communicated by the motion of the heart and vascular system to the different organs, may be deduced from the naturally deficient vitality of colourless parts, and their increase of the phenomena of life and sensibility under the application of stimuli, inflammation, &c.

INFLUENCE OF THE DEATH OF THE HEART ON THE BODY. The first in the order of effects is the death of the brain and all the functions of animal life. Second, the interruption of organic life; diaphragm and intercostals inexcitable; heart incapable of propelling the blood; mechanical and chemical phenomena of the lungs increased in consequence. Secretion, exhalation, &c. gradually cease. The colourless parts discover the signs of death latest, because the organic sensations and motions do not require the existence of a common centre; but to animal sensations and actions the cerebral influence is absolutely essential. By this mode, wounds of the heart and great vessels, polypi, ligatures, compression, and passions of the mind, induce death; viz. by stopping the action of the heart, and thence that of the brain.

Syncope from the last cause operates *directly* on the *heart*, and not as Cullen supposed, on the brain. It has been assumed that the passions never affect the animal life but *indirectly;* and the expression and phenomena of the affection are precordial, not cerebral. The brain exerts no *direct* influence on the heart. Break off all nervous communication between them, and no disorder of circulation appears. Intercept the channel of blood, and the brain exhibits extraordinary phenomena. Syncopes depending on morbid causes, present the same character. The distinction between *syncope*, *asphyxia*, and *apoplexy*, is, that the first kills the *heart;* the second, the *lungs;* the third, the *brain*. It is rare for the heart to die first, as happens in some disorders of this viscus. Death oftener commences by the lungs, than by the heart or the brain. In all deaths commencing by the heart or great vessels, the lungs are found invariably empty; for the circulation being suddenly interrupted, time enough is not allowed to fill the vessels of the lungs.

INFLUENCE OF THE DEATH OF THE LUNGS ON THE HEART. The death of the lungs may arise from the cessation of their mechanical or chemical functions. The former happens from paralysis of the diaphragm and intercostals, the latter from suffocation. All physiologists admit the possibility and fatality of a mechanical obstruction to the pulmonary circulation, *but this is never owing to, and never can be produced by collapse, or distention of the cells.*

PROOFS. The air being completely exhausted from an animal's lungs by a syringe introduced into the trachea, the blood flows as freely from a wound in the carotid as before. 2d. The sides of the chest being opened to an extent sufficient to produce a complete collapse of the lungs, the experiment has the same result; and the saltus lasts, in both cases, for some minutes. 3d. The facility of the circulation under aqueous, purulent, and sanguineous effusions in the chest compressing the vesicles. Some have supposed over distension by air in a state of rarefaction equally an obstacle to circulation. This opinion is equally groundless. PROOF. Inflate the air cells to the utmost of their capacity, retaining the air by a stop-cock upon the tube, and the experiment of opening the carotid will be attended with the same result.

Chemical phenomena. These are, not the deficient excitement of the left ventricle by the black blood, as

Goodwyn supposed, but the effect produced by the circulation of black blood upon all the organs of the body. *Proofs that the internal surface of the left ventricle is excitable by black blood.* 1st. The heart continues to act in animals asphyxiated for some time after the total cessation of the animal functions. 2d. Close the trachea of an animal, or exhaust the lungs of air, an artery being opened; the blood is seen to darken by degrees, and having acquired the truly venous character, is still jerked out with a force equal to that of florid blood. 3d. Inject venous blood, or hydrogen, or carbonic acid gas, (in containing which it differs from arterial,) into one of the pulmonary veins, and neither will be found to act upon the ventricle as sedatives; nay, they will frequently re-establish its contractions. The energy of action in the arteries is as little diminished as in the heart, and if their functions were arrested from no other cause, the circulation would go on, if not with equal, at least with sufficient force. The black blood acts upon the fibres of the heart as upon all other parts; i. e. as a *sedative*, and would put a stop to its action as quickly, even during the passage of arterial blood through its left cavities. This paralysis of the fibre is most probably produced through the medium of the nerves. This effect being the same on both ventricles, why is the right the *ultimum moriens?* If the left died first, as Goodwyn supposed, it, and its corresponding auricle, should be found most distended, and the congestion commencing from this point, should extend through the lungs to the right side. But the contrary is the fact, and the arteries contain as much as the left ventricle, which is scarcely any. The famous experiment of Haller, by which the left ventricle was made to beat longest, proves, not only the stimulant power of black blood, but that the later action of the right ventricle is owing to its presence and quantity.

When death is sudden, the right side of the heart is not distended; when more gradual, the right side is always turgid. The reasons are, 1st. The increased volume of blood returned by the veins; the venous blood not stimulating the organs of secretion, and consequently leaving their functions unperformed, and returning with the mass. 2d. The venous blood circulating in the bronchial arteries producing the same sedative effect on the lungs as on other organs; while from the deficiency of oxygen, (which is to the air-cells what food is to the stomach,) the cells are not expanded. 3d. The venous blood producing a similar effect on the walls of the heart, so enfeebles its contractions, that it cannot surmount the resistance set up by the lungs. *Additional reasons for the inequality of fullness in the two sides* are, 1st, The small quantity of blood which enters the left cavities. 2d. The greater propulsive power of the arteries than the veins, though subject to the same debilitating cause. 3. The smaller resistance offered by the capillaries than by the lungs, the latter being deprived of their habitual excitation from the atmospheric air, in addition to the circulation of black blood in their nutrient arteries. Two remarkable phenomena are consequent upon the resistance of the capillaries, though weaker than that of the lungs. 1. The stagnation of blood (generally fluid and uncoagulable) in the arteries. 2. The livid colour of surfaces (particularly vascular surfaces) from their engorgement with black blood. This is not, as writers assert, owing to a reflux of blood into the extremities of the veins, but to its presence in the small arteries from the impulse communicated by the Aortic Ventricle. PROOFS. Lay open the abdomen of an animal and deprive him of air, and the red vessels of the peritoneum change first to brown, and then to black; suffer him to inspire, and they again become florid red, and this alternation may be produced and repeated at will. Here no reflux can be suspected from the right auricle, the system of the vena portarum being independent of the great venous system.

The spleen is the only organ resembling the lungs in its susceptibility of great varieties in the quantity of its blood. These depend chiefly on the state of the liver, which is in local relation to the spleen, what the lungs are to the right auricle and ventricle.

The sensibility and contractility of the internal or vital organs remain for a time after sensation and motion are extinct; because the former do not depend on a common centre, but animal actions cannot survive the brain.

INFLUENCE OF THE DEATH OF THE LUNGS ON THE BRAIN. In the interruption of the chemical phenomena of the lungs, black blood is sent to the brain, which acts deleteriously on that organ, and deprives it of the excitement necessary to its action. PROOF. Inject blood from the jugular of one animal into the carotid of another; its animal life is entirely suspended, respiration and circulation become irregular, and the animal dies in about half an hour. The excrementitial fluids, ink, oil, wine, &c. produce the same effect. Its noxious agency is upon the brain, and not upon the internal surface of arteries, for an injection of venous blood into the crural arteries is never *mortal*, though it occasions something like a paralysis of the muscles.

That the atony of the cerebral system is owing to the nature of this fluid, is shewn by the continuance of the regular movements of the brain during the state of asphyxia. Since the mechanical stimulus of life exists, death must be the consequence of failure in the chemical; for if a cerebral affection co-exists with the state of asphyxia, death supervenes more quickly. A further proof of the fatal contact of black blood with the brain, is derived from the experiment of injecting blood from the carotid of an asphyxiated animal into that of a healthy one. There are two states or periods in asphyxia. 1. The suspension of the cerebral functions only, in which life may be recovered by restoring the arterial circulation. 2. The stoppage of the circulation and alternate motion of the chest, in which the venous blood has produced death of the vital organs.

After injecting venous blood by a syringe, the infusion of arterial produces only some feeble symptoms of excitation, and the animal is *irrecoverable.* The blood taken from a vein, is likewise more prompt and more certain in its effects, than blood darkened in the artery during obstructed respiration. The phenomena of asphyxia viewed as a disease accord with these results, that is, they prove the more immediate influence of black blood upon the brain than upon the heart, and this inequality of influence explains the difference in the period of cessation of the two lives. In cold-blooded animals its effects are less strongly marked, and its operation slower upon the vital organs. The death of the brain is however only an insulated and partial result; the life of the body ceases solely from the contact of black blood with the vital organs.

INFLUENCE OF THE DEATH OF THE LUNGS ON THE OTHER ORGANS OF THE BODY. The phenomena of the production of black blood from the interruption of the chemical functions, are best observed by adapting a tube with a stop-cock to the trachea of an animal whose heart resembles the human, and making a small opening in an artery of the middle size. If the tube is shut immediately after inspiration, the blood begins to darken at the end of thirty seconds, and after a minute and an half perfectly resembles that of the veins. If the tube be closed after expiration, the change of colour is more rapid, and this in proportion to the force of expiration. If the air be withdrawn from the lungs by an exhausting syringe, it darkens suddenly instead of gradually, and if air be thrown into the lungs in larger quantities than it is absorbed in inspiration, the colour does not sensibly change in less than a *minute*, and is not completely venous in less than *three*. Hence the period required for the change of blood from red to black, is in the direct ratio of the quantity of air contained in the lungs. The change from black to red is sudden and not gradual, where the tube is opened after having been closed for some minutes. The change is less manifest when air is thrown in by a syringe than after a natural inspiration, from mixing with the foul air already in the vesicles. The rapidity of the change proves that the principle which produces it must traverse the walls of the cell, and cannot be communicated circuitously. When the heart has not experienced the destructive influence of black blood, it sympathises in the excitement of the lungs by inflation; but when this blood has once penetrated its tissue, it is dead to sympathy as well as to direct stimuli. The difference in the intensity and rapidity of coloration, during the respiration of hydrogen and carbonic acid gas, is not observable. It is black in from four to five minutes. The longer time required to produce the change in this case than in closing the tube in the trachea, is owing to the agitation which the air undergoes in the lungs and bladder alternately, presenting successively the respirable points to the capillary orifices.

The blood rendered black by the interruption of the chemical changes in the lungs, penetrates all organs, and circulates for some time in their arteries, as is proved by examining during suspended respiration, muscles, nerves, skin, membranes, and granulations. In some parts the capillaries do not receive it, and they remain colourless, in others they do admit, but are unequal to pass it; in others again, it passes through them and the corresponding veins as readily as arterial blood, and meets with no obstacle until its arrival at the capillaries of the lungs. Experiment does not discover any sensible difference between the blood of the umbilical artery and veins in the fœtus. It is equally dark whether the respiration of the mother continue or not. This circulation of black blood explains the phenomenon of its frequent appearance in the arteries after death. The functions of the lungs are in almost all cases embarrassed, and terminate before those of the heart. Hence the blood performs some rounds of the double circulation, after it has ceased to receive the influence of the air, and therefore remains ultimately dark in the vessels. Hence also, the dark colour of the blood in the cells of the viscera, in the mucous membranes, in apoplectic extravasations, and in the capillaries of the skin.

Blood is then unfit to support the action and life of the organs which it penetrates, after the chemical functions of the lungs have ceased. The effect of the contact of red blood is to excite and exalt the actions of life; black blood on the other hand produces a local prostration of their powers. Compare a florid with a livid countenance, a phlegmonous with a petechial eruption, a scarlet surface of granulation with one that is pale or brown, &c. &c. The sedative effect is propagated, as we have seen, *simultaneously* to the brain, its nerves and voluntary movers, to the heart and lungs, and reasoning by analogy, (for this is insusceptible of proof) to the organs of secretion, exhalation, and nutrition. But though the death of the other organs coincides with that of the brain and heart, if it were possible for these two organs to receive red blood while black penetrated the remainder, the latter would cease to act while the former continued their functions. Asphyxia then, is a *general phenomenon*, which is developed at the same instant in all the organs of the body, and which is not very peculiarly apparent in any.

INFLUENCE OF THE DEATH OF THE LUNGS ON THE BODY. If the mechanical functions are impeded as by a ruptured diaphragm, &c. all the functions animal and vital are excited to increased exertion. If the chemical functions are obstructed, as by strangulation, &c. they are gradually suspended, and soon terminate finally. The symptoms of asphyxia are subject to considerable varieties depending upon its cause. All these varieties may be comprehended in two classes; those which supervene *from want of respirable air*, and those in which, joined to the first cause, a *deleterious fluid is introduced into the lungs.* There is little difference in the symptoms of the first class except in their duration, which of course varies with that of the intercepting cause. But in asphyxia from noxious airs, the privation of respirable air, and the presence of the deleterious fluid co-operate. If the poison is very active, death ensues before the effect of the black blood is felt; if slow, the animal dies from the latter cause. The variety of the symptoms is in part, referable to the nature of the poison, and partly to that of the individual, in relation to temperament, susceptibility of the lungs and brain, &c. But the varieties bear upon the *intensity* and not upon the *nature* of the symptoms, which are pretty constantly the same, except in degree.

Noxious airs must act either on the nerves of the lungs, or on the body through the medium of the circulation. There are numerous instances of the marked effects of aerial substances on the pituitary and other mucous membranes, but none of their producing sudden death. The rapidity of this event in asphyxia from mephitic vapours anticipates the influence of the unchanged blood, and we must therefore suppose that they are carried across the lungs to the left side of the heart, and impregnate the blood destined for the brain; which is an immediate and inevitable cause of death. The lungs of an animal being inflated beyond their ordinary capacity with any aeriform fluid, and this being retained by a ligature on the trachea, the blood drawn from an artery is observed to be frothy, and filled with air bubbles. The presence of hydrogen, if this air has been injected, may be detected by presenting to it a lighted taper. This affords a proof of the passage of air into the blood through the lungs, in addition to that of healthy respiration, &c.

It is upon the *brain and nervous system* that deleterious substances act through the medium of the blood. The symptoms are those of irritation, or compression of the brain, as spasm, or torpor. The animal life is always suddenly interrupted before the organic; and symptoms of the injury sustained by the former, uniformly attend recovery.

In a majority of diseases, death commences by the lungs. Breathing is embarrassed, and the blood passes unchanged into the arteries. The morbid signs of it are, *black blood in the arteries*, and *engorgement of the lungs*. When death commences by the heart, as in syncope, &c. we find *vacuity of the pulmonary vessels, and red blood in the left ventricle.* When by the brain, *vacuity of the pulmonary vessels, and black blood in the left ventricle.*

INFLUENCE OF THE DEATH OF THE BRAIN ON THE LUNGS. The brain has no sooner ceased from action, than the functions of the lungs are interrupted; because the action of the brain is essential to that of the lungs, intercostals, and diaphragm. The brain however exercises no *direct* influence over the lungs, as is proved by the division of the par-vagum and sympathetic nerves; but acts upon them *indirectly*, through the medium of the diaphragm, intercostals, and their auxiliary muscles. The mechanical, and next the chemical functions of the lungs are arrested.

Respiration is a mixed action, placed as it were between the two lives; related to the animal by its *mechanism*, and to the organic by its *chemical* phenomena. In proportion as the cerebral organization is imperfect, so is the pulmonary function limited. Hence in the mammalia and birds, the function is more perfect than in reptiles and fish. The nervous system of animals respiring by the skin is least perfect; and in those which have no nerves, the function does not exist. The relation between the brain and lungs is reciprocal, particularly in mammalia and birds. The first favours the entrance of the air into the second; and the second, by its effect on the blood, supports the action of the first. In insects, where the air penetrates the open pores of the surface, respiration seems to have no cerebral connection, and belongs altogether to the department of organic life.

INFLUENCE OF THE DEATH OF THE BRAIN ON THE HEART. Pathological observations, mechanical and galvanic excitants of the cerebral and cardiac nerves, concur to prove that the brain exercises no *direct* control over the heart. The lungs are the intermediate organs affected, and their influence on the animal and organic functions has been explained. The death of the brain from that of the heart is a consequence *direct.* The death of the heart by that of the brain is a consequence *indirect.*

INFLUENCE OF THE DEATH OF THE BRAIN ON THE OTHER ORGANS OF THE BODY. The internal functions are so far abstracted from the influence of the brain, that the interruption of the former is never an immediate consequence of the death of the latter. The life over which the cerebrum presides, may be suspended by sleep, or injury, but the organic functions remain the same. From the phenomena of paralysis, spasmodic and convulsive diseases, brainless monsters, and the tribes of animals wanting a nervous system, are deduced the proofs of this statement. The origin of the sympathetic is exclusively in ganglia; it is not a continuous nerve, but an assemblage of small nervous systems, each having a ganglion for its centre, and each independent, although ordinarily communicating with others, and with the spinal marrow. The ganglia are the *centre* of the *organic,* as the brain and spine of the *animal life.* Though in some instances organic agents are supplied by cerebral nerves, their functions are equally independent of *direct cerebral* influence. Experiment confirms the fact of a vast difference in the relative excitability of the muscles of animal and organic life; and likewise establishes, that the brain does not influence in a *direct manner* the organs and functions of internal life; and consequently, that the interruption of these functions in lesions of the brain, is not an immediate effect of such lesions. The interruption of the organic functions is therefore an *indirect* effect of the cessation of cerebral action. The difference between death from asphyxia, and from injuries of the brain, is depending on the greater or less violence of the injury, and the suddenness of the asphyxia. They approach, or recede from each other, according to the quickness or slowness of the cause which operates to produce them. In general, there is a nearer relation between the two modes by which death of the *brain* or *lungs* produces that of the other organs, than between either of these modes and that which is a consequence of the death of the *heart*.

INFLUENCE OF THE DEATH OF THE BRAIN ON THE BODY. The following is the series of results. 1st. Annihilation of the cerebral action. 2d. Sudden cessation of sensation and voluntary motion. 3d. Simultaneous palsy of the diaphragm and intercostals. 4th. Interruption of the mechanical phenomena of respiration. 5th. Annihilation of the chemical. 6th. Passage of black blood into the arterial system. 7th. Retardation of the circulation from the contact of black blood, and the absolute immobility of all parts, particularly of the chest. 8th. Death of the heart, and stoppage of the general circulation. 9th. Simultaneous interruption of organic life, especially in parts habitually penetrated by red blood. 10th. Abolition of animal heat, the collective product of all the functions. 11th. Consequent termination of the action of the white organs, which are the slowest to die, because the juices which nourish them are most independent of the great circulation. Organic sensibility and contractility remain for a certain time in the muscles; galvanic excitability especially in those of animal life. The duration of the organic properties is proportioned to the rapidity of dissolution.

ART. II. *A Treatise on the Anatomy, Pathology, and Surgical Treatment of Aneurism; with Engravings. By Antonio Scarpa, Professor of Anatomy and Practical Surgery in the University of Pavia; Member of the National Institute of the Italian Republic, of the Royal Academy of Berlin, of the Royal Society of London, and of the Medical Societies of Paris, Edinburgh, &c. Translated from the Italian, with Notes.* By John Henry Wishart, Fellow of the Royal College of Surgeons, and Member of the Royal Medical and Chirurgical Societies of Edinburgh. 8vo. Edinburgh, 1808. pp. 503. Mundell, Doig, and Stevenson, &c. &c. Price 15s.

THERE is no subject, not even excepting those of hernia and injuries of the head, to which modern surgeons have devoted themselves more anxiously than that of aneurism; nor is there any on the history of which they can found a more secure claim to the honour of having improved the science and art of their profession. Till the publication however of the Treatise of Mr. Bell, the Essay of Dr. Jones, and the volume before us, the subject can scarcely be said to have undergone a comprehensive review; which we are inclined to attribute, less to superior genius in these recent writers, than to the advanced state of knowledge when they engaged in the investigation.

The belief of the older surgeons, that the great arterial trunks of the extremities could not be tied without death in the limb below, was rational, and their consequent timidity just. Bold conjecture, which reason could hardly sanction, and daring practice, which necessity alone could justify, seem to have been among the first steps of the gradually progressive improvement. The first successful cases were rapidly followed by others; and these by numerous works, which though proceeding from men of acknowledged talents, contained details of experiments on the powers of the animal economy, rather than reasoning founded on a knowledge of those powers. The courage of surgeons increased in proportion to their experience; and has now nearly, if not absolutely, reached those limits beyond which it would be dangerous and culpable rashness. The principal arterial branches have been tied as they issue from the trunk of the body; the humeral, axillary, and carotid; the femoral, inguinal, and external iliac; with such events as have diffused this courage beyond the great metropolitan hospitals to the skilful and well-practiced anatomical operators in distant parts. This obstruction of the larger arteries, though free from imminent danger of gangrene, has nevertheless been found insecure. Trials therefore have been made with ligatures of various forms, and with various complications; and details of the effects of each have been made the subjects of numerous essays. These circumstances, together with the multiplied experiments and adverse opinions of several of the first surgeons in Europe, concerning the general subject of hemorrhage from divided arteries, led to considerable confusion, and to proportionate self-sufficiency and altercation. This state of things naturally called for the exertions of dispassionate and cautious men who might go over the whole, present a general view of the successive improvements, try each controverted point by the test of their own observations, and give the sanction of cool judgment to what might appear to be the truth. Such was nearly the state of facts when Jones and Scarpa wrote; and such has been their office. Their works are the most valuable of those which we possess, not because they are intrinsically the most able, but because they are the latest writers. They have had the advantage, not merely of being last in point of time, but of approaching the end of their subject. Each traveller in a new path of science does not perform every step of the arduous progress himself: it is divided into stages, and he to whom the most distant is allotted, often gratuitously bears those honours and emoluments of which a just distribution would have assigned him only a part.

> "'Tis the last key-stone
> That makes the arch: all else that there are put
> Are nothing, till that comes to bind and shut."

Yet we would by no means be understood to form a low estimate of those qualities, which are requisite not only to give a comprehensive view of a busy period in the history of science, but also to form a just appreciation of the various results. Sagacity is a faculty which is perhaps equally rare with inventive genius; and probably more useful, inasmuch as it is less liable to be bewildered in the mazes of its own ingenuity, and to mislead or dazzle others by its brilliance. Ardour in inquiry, and coolness in decision; comprehension to grasp and compare great objects; and accuracy to scrutinize and estimate minutiæ, are essential to its composition*. Among recent writers on medicine we know not where to look for it. It is not to be found in the phlegm of Cullen, or the fire of Brown; not amidst the coruscations of Darwin, or the scintillations of Beddoes. Even Fordyce is perpetually ingenious at his own expense. Nay, we should hesitate in giving this praise to Currie, had he not proved the possession of comprehensive views in his "Letter Political and Commercial," as well as of scientific accuracy in his medical writings.

The exertion indeed of this rare faculty, is perhaps more difficult in subjects connected with medicine, than in any other science. Many of the objects to be considered are extremely minute, and their actions mutable; whence many false though imposing doctrines have sprung, from which it is essential that the mind should be freed before it can exert itself with vigour and effect. We have said that the opinion of the older surgeons, that the large vessels of the extremities could not be tied without mortification, was rational; for an expectation of those powerful and varied actions which take place in the constitution under disease, and still more remarkably after injuries, could only be authorized by an experience of their previous occurrence. The phenomena of physiology differ from other phenomena in physics chiefly by their fluctuations. They do not consist in a succession of simple and unvarying events; but comprise the most complicated of nature's works. Some of the actions of the living system depend on comparatively rare contingencies for their production; and in consequence, when they occur they seem irregular, and are less easily traced. To account for their occurrence, some have had recourse to the more obvious laws of general physics; whilst others, disgusted by the futility of these, have classed them at once under the indefinite head of vis conservatrix, or vis medicatrix naturæ. From their frequent tendency indeed to an obvious and beneficial result, they sometimes appear to proceed less from consistency with general laws, than from obedience to an immediately presiding intelligence. Though from what we have said we may be supposed in some degree to admit the existence of these sanative powers; yet we object strenuously to such terms, and to the use which has been made of them in medical discussions. If they be meant to refer to a general principle, we object to them as false; and if they be not, we object to them as useless. The notorious consequence of their use has been, that men indolently satisfied with names of undefined import have ceased to inquire, or have inquired listlessly, into the train of events which is thus set apart. We deem it most consistent with philosophy to believe, that in this as in other parts of science, the usual and unusual phenomena are alike portions of the same general laws, and that they constitute a train of events in successive dependance on each other. When therefore one more prominent than the rest appears, our obvious duty is to trace its connection with those which ordinarily occur, rather than to suppose it to be the first of a new and independent series spontaneously commenced. The powers of nature suddenly called into unusual action are more conspicuous in many of the phenomena of aneurism, and its modes of cure natural and designed, than in almost any other state of the body. And although the reasonings of some great surgeons* from Hunter downwards, have been materially influenced by such doctrines, yet they have in no instance been so deeply tainted by them as those of physicians. In surgery the successive processes pass for the most part under the eye of the surgeon, and hence prevent the necessity of his having recourse to remote and indistinct solutions.

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* As an example of what we mean, we may specify the work of Horne Tooke, who, in his Analysis of Language, has shown himself amply entitled to the praise of sagacity, largely as we have defined it.

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* See Mr. Hunter's account of the coagulation of the blood in order that it may answer the necessary purposes of solidity. We remember to have heard an eminent teacher publicly explain the *cause* of an universal transposition of the viscera in the following manner: "When the viscera are transposed, nature makes that transpo-

Neither the work of Dr. Jones, nor indeed that of Professor Scarpa, is disgraced by indolent and premature generalizations; but the former corresponds more correctly with our ideas of what such a work ought to be, than the latter. It is more precise and conclusive; but at the same time it is much more limited in its subject. The author has followed the train of experiments suggested to him with zeal and accuracy, and having fully detailed the results, has given us nearly a definitive book. The subject of Professor Scarpa is much more extensive and less advanced; and in consequence, his volume though in many respects much more able is less satisfactory.

This work was originally undertaken in consequence of some questions, proposed as the subjects of a prize essay in 1798, by the Society of Medicine of Paris. The questions were;—

In what cases of aneurism will internal medicine, regimen, and rest, probably prevent the necessity of surgical treatment?

When surgical aid is required, in what cases will compression suffice; and when is it expedient to have recourse to operation?

When is a single ligature superior to two;—viz. one above and one below the sac? In what cases is it adviseable to open and remove the sac; and in what cases is it better to leave it?

These questions embrace all the points of practical importance connected with the subject; and it is obvious, that they cannot be answered without entering into a copious physiological and pathological detail. The present author did not present a memoir for the prize, although he had begun to write for that purpose; partly it appears because he was accidentally prevented; but chiefly because he could not discuss the various parts of the subject so amply as he wished, within the necessary limits. The comprehensive work before us has been the result.

A large proportion of the first part of this volume is occupied by details of the anatomy in health; in which minute descrip-

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sition total, because if it were partial, the functions of the system would be impeded or interrupted; whereas by a mere change of place in all, their actions continue unimpaired."

tions are given of the anastomoses which take place in the neighbourhood of the more important aneurisms, in the upper and lower extremities. This portion of the translated work is infinitely less valuable than the same part of the original; because the descriptions are not accompanied by corresponding engravings. If it be true that anatomy cannot be learned from books, verbal descriptions without plates must be nearly incomprehensible, and insufficient to give even general ideas approaching to correctness. The figures in the original are among the most minute and satisfactory that we have seen, and we regret that they have not been republished. The translator says in excuse "It was at first intended, in imitation of the author's plan, to have given a series of plates, or outlines, to have illustrated the anatomical descriptions of the arteries; but this would have added so considerably to the expence of the work, that it was thought expedient to omit them."* Any extra-size or extra-elegance we should very willingly have spared on the score of expence, but we think the total retrenchment injudicious, as it renders a large and important part of the work of little comparative importance.

The omission is the more to be regretted, as the minute anatomy of anastomoses is seldom fully delivered in general books upon the subject. Still less usual is it for such minutiæ to be attended to by young surgeons in a dissecting-room; or even to be given, except in a very cursory manner, by their teachers in an anatomical theatre. Were they given indeed they would not be attended to; for such minutiæ can only interest those who are studious, as in the present instance, to illustrate some point of speculative or practical importance The descriptions, as they now stand, may be subjects of interest to those who have already thoroughly studied the subject, or those who have good plates before them or unusually minute preparations. To others, (and the author professes to write chiefly for the young in his profession,) they can produce little else than weariness and confusion. They will not however be read by more than one tenth part of the readers of the work. The two plates which are given illustrate their subject perfectly, and we therefore rejoice that they have been copied; yet they might have been spared with less disadvantage, because that which they are intended to show can be readily conceived without their assistance.

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* Advertisement.

The three following chapters relate to the anatomy of aneurism, concerning which the author maintains opinions that are not now generally received; and to the exposition and illustration of which he has therefore devoted perhaps more than an adequate share of his volume. It must be well known to our readers, that very different opinions respecting the causes and nature of this affection have prevailed at different periods. The opinions of men during the more remote ages of physic can be of little consequence except as subjects of curiosity, at the present day, for they were not founded on dissection. They seem however to have been somewhat similar to those of Professor Scarpa.* "Oritur dilatatio," says Ætius as quoted by him, "aut dum sanguis et spiritus ex arteriis prosultant, aut dum oscula ipsorum aperiuntur, aut dum rumpuntur. Sanguis autem et spiritus paullatim excreti sub cute colliguntur." The first who gave any thing that could be called precision, or the confirmation of dissection to this doctrine, were Hildanus and Sennertus, of whom the former in his account of aneurism from wound observes, that the outer coat only of the injured vessel heals, but being insufficient of itself to resist the impetus of the blood gradually yields, and swells into a pulsating tumour:—and the latter extends the doctrine from the case of a wound to all aneurisms, whether arising from disease in the arterial coats, or from sudden exertion. The difference between the more loose and ancient opinion and that of Sennertus, seems to have been this; the older writers supposed that all the arterial coats were ruptured, and the blood effused; whereas Sennertus maintained, referring to dissection in support of his doctrine, that one remained entire.

Some time before Sennertus, an opinion contrary to that just noticed had been published by Fernelius, and rested probably on the same basis of conjecture and lax reasoning, on which the opposite doctrine had originally been supported.

"Any one not acquainted with the history of medicine would perhaps suppose, that Fernelius was induced to publish this new theory; quite different from that of the Greek and Arabian physicians, in consequence of repeated researches on the dead subject, and of exact comparisons of the nature of the proper coats of the artery and of those which constitute the aneurismal sac. But if they would wish to be fully informed of this, they will find that the theory of

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* This has been the subject of dispute, but we believe the statement of the author to be *generally* correct. "Minime implicari hic volumus ea questione, quid Græci medici aneurysmatis vocabulo proprie significarent." Morgagni.

Fernelius, far from having been deduced from observation on the dead subject, was only the result of his own imagination."*

Dissections however seem soon to have been resorted to in support of this opinion; though we are willing to allow that they were by no means on a level with those of our author in point of minuteness; and indeed as he has not noticed them, he probably regarded them as unworthy of notice. "Vesalius cum in dorso circa vertebras tumorem quendam pulsantem animadvertisset, ea secutus quæ in externis aneurysmatibus antea deprehenderat, ibi esse dixit aneurysma ex dilatatione aortæ arteriæ. Sed tamen non ante medici verum dixisse Vesalium pro certo sciverunt, quam cum ægro mortuo eam arteriam dilatatam adeo invenerunt, ut ovi struthiocameli magnitudinem fere æquaret." "Conspectum id anno est 1557, sicque additum pondus iis quæ Fernelius scripserat tum ubi docuit cordis palpitationem sæpe arteriam foras dilatasse in aneurysma contracti pugni magnitudine, in quo et tactu et visu conspicua erat pulsatio; tum ubi, externis aneurysmatibus propositis, hæc subjecit; fit et nonnunquam aneurysma in interioribus arteriis maxime sub pectore, circa lienem et mesenterium, ubi vehemens sæpe pulsatio animadvertitur."† It appears therefore, that when arterial aneurisms were first observed in the 16th and 17th centuries, they were regarded as proceeding, not from rupture, but dilatation of the vessels in which they occurred.

Great surprise is expressed by Scarpa, that the doctrine of Fernelius was not only well received by some men of eminence in his day, but that it has been handed down as the legitimate doctrine, and admitted as such by a majority of the surgeons of the present time, and even by men of distinguished anatomical science. We must confess that in this we see no subject of wonder; even supposing, what we are by no means unwilling to suppose, that the doctrine of Scarpa, or what is in truth the same thing, the doctrine of Sennertus, is founded on the genuine anatomy. The opinion of Sennertus we have seen is essentially different from that of the older physicians, which was prevalent at the time when Fernelius wrote. These physicians supposed the artery to be burst, and the blood to be poured out under the muscles or the skin. When therefore after the publication of a contrary opinion, it was found that in fact aneurisms occurred within the cavities of the thorax and abdomen, where there is no substance of muscular flesh, no strong bands of ligament or skin to circumscribe the blood, when effused; that these internal tumours were covered by the same continuous external membrane which enveloped the sound artery; that the sac was of the same thickness with the healthy vessel; and that the interior of the tumour exhibited a mass of intricacy which rendered accurate dissection at all times difficult, and sometimes impossible; it is not to be wondered at that they adopted this opinion, extended as it was to every instance that occurred.

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* Scarpa, page 63. † Morgagni, Epist. 17.

The doctrines of the present day admit two species of this disease, viz. the true aneurism, in which the arterial coats though diseased are entire; and the spurious, in which they are ruptured. Mr. Benjamin Bell in his account of this subject observes, "by the term aneurism was *originally* meant a tumour formed by the dilatation of an artery; but modern practitioners make the term apply not only to tumours formed in this manner, but to such as proceed from blood effused from arteries into the contiguous parts." Two varieties are given also by Mr. John Bell, although he previously says, "I will boldly neglect those insignificant definitions which never can circumscribe a subject like this; definitions which no sensible student will value, and no sensible teacher attempt. For decision and judgment in operating or in consulting, depend not on curious and nice distinctions, but on universal knowledge."*

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* Universal knowledge! what idea can Mr. Bell possibly have annexed to this term as contrasted with knowledge of particulars, of minutiæ, of nice distinctions? Was it, we would ask him, for want of this *universal knowledge* that he did not write the chapter on the structure and actions of arteries, announced in the contents of his second volume of anatomy; or was it not for want of *minute acquaintance with some common details of general science?* We presume that this phrase is in fact, nothing more than an example of those turgid, random expressions which distinguish this writer. As an anatomical demonstrator, we do not know the equal of Mr. Bell, and in this capacity we regret that he is no longer to be heard. With the body before him, in spite of his hatred of circumscription, he could not wander far; and his uncommon dexterity and great animation gave an impression and interest to his subject, which we never felt out of his class-room. As a writer on surgery we can by no means give him equal general praise. His egregious vanity, his constant study of point, and fondness for effect, induce him perpetually to mislead his unwary readers by extravagance. His general style and manner give us rather the idea of a petulant coxcomb, studious to display his own perfections in contrast with others, than of a serious, sober man, pursuing truth through details of the various forms of misery, pain, and death, which are incident to his fellow beings.

"What! will a man play tricks—will he indulge
A silly, fond conceit—
Or will he strive to dazzle me with hopes,
And play his brilliant parts before my eyes
When I am hungry for the bread of life!"

He is indeed bold enough not to give his definitions *totidem verbis*, but to occupy at least 20 quarto pages with details which have relation only to definitions, or in other words, to *curious and nice distinctions* of the varieties to be found in this disease. Mr. Bell then divides aneurism into two kinds, which he defines after his own manner to the following purport. 1. Natural aneurism, or that which arises spontaneously and consists in a dilatation of the coats of the vessel. 2. False aneurism, or that proceeding from rupture, in which the inner coats are lacerated, and the outer coat or sheath distended. After a description of the first species, he says, "these successive aneurisms are found to be true dilatations of the artery; there is no breach of the arterial coats; the cellular, the muscular, the villous are thickened as they approach the dilated part, and may be fairly traced over the aneurismal tumour; it is on the tumour indeed that the several coats are most distinctly traced."* After quoting a second case, in which an aneurism had followed the forcible extension of a diseased knee, he adds, "In this instance some of these brittle ossifications had given way, or hurt the adjoining parts; the remaining coats of the artery had dilated into a true aneurism; the artery had certainly been injured, though not entirely lacerated."† Again he writes, "When, as it often happens, a man in the very time of falling, as in the instance of a blow or sprain, feels pain in the ham; when he observes that he has, from that moment, considerable pain in laying his leg across; when on the third or fourth day he feels distinctly the pulsation, pain, and lameness which are the peculiar signs of aneurism, what reason can there be for doubting that the artery is burst? none surely, but the slowness with which the aneurism grows."‡ After answering this objection in detail, he proceeds, "Thus we perceive that the increase of such an aneurism, when the artery is absolutely broken across, is naturally slow, that the resistance is uniformly great; that the artery does not lie, as in our preparation glasses, or on the dissecting table, insulated and unconnected, but is surrounded with cellular substance; its rupture is walled up with coagulated blood; its canal is open to the circulating fluids, and there is for a long while a firm pulse, even in the artery which is desperately wounded, provided it be not entirely cut across."* Whether the second and third descriptions were meant to refer to a similar state of the artery, we were for some time doubtful. We at first thought that the author meant in the second to represent the internal coat only as injured, and the muscular coat and sheath "the remaining coats" as distended; and that in the third he meant to describe the muscular coat also as torn, and the external sheath alone as forming the aneurismal sac; for in the former of these two cases, he said that the artery was "injured, but not entirely lacerated;" and in the latter, that it was "absolutely broken across." In a note however explaining a sketch of the second case he ultimately declares, that he has very little doubt that the vessel was broken across, and as in the last case quoted, that the sac was formed entirely by the sheath.

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* Principles of Surgery, Vol. 1. page 314. † Ibid. page 316.
‡ Ibid. page 317.

Such appears to us to be a fair though hasty sketch of the different opinions which have prevailed respecting this disease. We think that it would be impertinent to quote a greater number of our cotemporary writers, as the coincidence of the hostile Bells sufficiently marks the currency of the doctrine in which they agree. If we may be allowed to refer to our own general impressions, as received from our teachers and from the usual language of surgeons, we would state the following to be the most prevalent opinion at present. Aneurism though it sometimes occurs in a sound artery from rupture in consequence of great exertion, commonly proceeds from a diseased state of the vessel,† which becoming weaker is sometimes burst, and more frequently distended by the usual or unusual force of the circulation. The primary disease of the arterial coats is not mere laxity, but a change of structure; so that when a large aneurismal sac is dissected, very slight remains of the original structure can be found.

Against such opinions Professor Scarpa has argued long and ably. He maintains that in *every* instance of this affection, *without exception*, the proper coats of the artery are burst.

"One of the *principal* objects which I have in view, in treating of this very important subject, was that of demonstrating the insufficiency of the doctrine which is commonly taught in the schools of surgery, with regard to the manner in which aneurism is formed. For after a very considerable number of investigations instituted on the bodies of those who have died of internal or external aneurisms, I have ascertained, in the most certain and unequivocal manner, that there is only one kind or form of this disease, viz. that caused by a solution of continuity or rupture of the proper coats of the artery, with effusion of blood into the surrounding cellular substance; which solution of continuity is occasioned sometimes by a wound, a steatomatous, earthy degeneration, a corroding ulcer, a rupture of the proper coats of the artery, (I mean the internal and muscular,) without a concurrence of a præternatural dilatation of these coats being essential to the formation of this disease; and therefore that every aneurism, whether it be external or internal, circumscribed or diffused, is always formed by effusion."

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* Principles of Surgery, Vol. 1. page 320.

† "The arteries near an aneurism are diseased to a greater or less extent in different persons; but I do not recollect one instance in which they were totally free from disease." BAILLIE.

This opinion is of course chiefly supported by details of the appearances on dissection of aneurisms in different arteries, and proceeding from different causes: and these details are illustrated by two very distinct plates; one of which represents the artery as torn on one side only, and the other shews it torn through its whole circumference. There are however some collateral statements and reasonings, of which it may not be improper to take some notice.

"The repeated investigations which I have made with regard to this important subject, authorize me to assert, that Hildanus and Sennertus had only a short step to take, to give to their theory concerning the proximate cause of aneurism, all that degree of demonstration and proof of which the subject is susceptible. This short step consisted in determining with anatomical precision, the relation of the proper coats of the artery with its external cellular covering; and in demonstrating by means of accurate dissections of arteries, both in the sound and morbid state, what share the proper and constituent coats of the artery had in the formation of the aneurismal sac; and what belonged to the cellular covering and the other adventitious membranes surrounding the affected artery. These are the circumstances which I now propose to detail, and to place, as far as lies in my power, in the clearest light.

"The covering of the artery which former anatomists, almost to the present day, have mentioned amongst the component coats of an artery, is by no means such as they have described it, but is merely an adventitious sheath, or cellular covering, which the artery borrows or receives in common with the parts in the vicinity of which it runs; by means of this cellular sheath the artery is kept in its place, as within a soft bed, and connected to the parts surrounding it, such as the membranes, the viscera, the nerves, the aponeurotic and ligamentous fasciæ, the muscles and periosteum. On cutting an artery across in its natural situation, it is observed to be enclosed in a sheath of soft, spongy, easily distended cellular substance, within which sheath the segment of the cut artery retires and conceals itself." "The pleura lies over the cellular sheath of the arch of the aorta, and over that of the thoracic aorta; and that of the abdominal aorta is covered by the peritoneum." "The great arteries of the extremities which run between the muscles, and under the aponeurotic and ligamentous fasciæ, are not covered, in addition to the cellular substance, by a smooth membrane, such as the pleura and peritoneum; but they are surrounded by a cellular sheath, which although it is here and there filled with an oily substance, may notwithstanding be demonstrated to be distinct from the membrane properly called adipose*."

Now we are willing to admit that every artery is clothed by layers of cellular substance, which connect it with the neighbouring parts, and we also admit what no one can be supposed to deny, that these layers are of primary importance. Yet we must confess that we can by no means comprehend of what great consequence it can be, to call these by one name rather than another. If it be true as a matter of fact, that the internal and muscular coats of an artery are lacerated in every case of aneurism, and if Sennertus discovered this truth, we cannot discern what step in addition has been made by the present author. Sennertus, according to Professor Scarpa, "guided by solid anatomical knowledge," decided that the external covering alone formed the aneurismal sac. It was not therefore left for any succeeding writer to demonstrate "what share the proper and constituent coats had in the formation of the aneurismal sac." Nothing in fact has been added to his account, as stated by Scarpa, but the change of an appellation. What was formerly called external coat is now called cellular sheath; a change which is productive of no advantage, but the rare one of allowing such forcible writers as Mr. John Bell, to speak at the same time with great energy and without impropriety. Under these definitions we can assert, that arteries in this disease are torn asunder, burst open, or absolutely broken across; assertions which under the old mode of speech could not have been allowed. We have been really chagrined by the importance given to this part of the subject, for it savours more of the disposition of certain tenacious dissectors of trifles in our own island, than of the mind of a great and scientific surgeon. We would ask too whether there may not be some risk of error and confusion, in calling that an *adventitious* membrane, which is *invariably* found to cover that to which it is said to be adventitious. On this subject the language of the first Monro pleases us more, and that of Dr. Jones the most. "All arteries are covered externally by cellular substance composed of very fine pellucid membranes, which are capable of being stretched even suddenly, to a great extent without breaking, and they collapse as quickly when the stretching force is removed." "All arteries are surrounded with such a substance as I have just now described, and therefore it may be reckoned one of their coats; though I must observe that the same kind of cellular substance is common to at least all the flexible parts of the body, where every little fibre is connected to another by the same contrivance*." This mode of speech gives at once a fair statement of the anatomical fact, and leaves the name by which we may denote it in its original state of insignificance. As this dispute however has arisen, it is now of some consequence that it should be settled, in order that the language of surgeons about the artery and its sheath should be uniform and not confused. It may be settled we think upon the following principle. When an artery with its natural coverings is divided, we see, as it were, two tubes; one retracted within the other. Let the coats of that which is thus retracted be called the constituent coats of the artery, and let the other be called the sheath. Which then is the external coat of the retracted tube? Scarpa says the muscular†, and Jones, the membranous‡, and we believe the truth to rest with the latter. If so, the description of Dr. Jones will be perfectly correct:—"The external coat anatomically considered is so simple, that many authors have thought it sufficient to say, that it is formed of condensed cellular membrane." "Although ultimately resolvable into this structure, yet it derives from the particular arrangement of its component fibres a characteristic appearance which distinguishes it from cellular membrane§."

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* Page 68.

In the year 1728, Dr. Nicholls published an account of experiments which he had made, to ascertain after death what are the comparative powers of resistance and extensibility of the arterial coats. By forcing air into the tubes of arteries, and urging it with considerable impulse, he invariably found that the external coat alone was sufficiently tough to form any large pouch; of the other coats, which in these injections were torn, the muscular was found to be the weakest. On similar experiments repeated by Professor Scarpa himself, and the results of which were always the same, he lays considerable stress, as affording important testimony in favour of his opinion concerning the pathology of aneurism. To this kind of reasoning we must decidedly object. Whether aneurism be formed by rupture or dilatation, can only be decided by dissection of the *morbid* vessel. The effects of distension on living and dead membranes may not differ materially from each other; but the distension of living and dead muscular fibres must produce results so totally dissimilar, that no reasonings founded on the one can be safely applied to the other. The resistance to extension made by a living muscle proceeds less from its mechanical connection, than from the synchronous contraction of all its fibres. No expectation could be formed by experiments on the dead subject, of the power of muscular contraction to tear the tendo Achillis, or to fracture the patella. In the living vessel, if the muscular action be insufficient to produce contraction, in opposition to the distending force, it will at least produce concurring action of the muscular fibres, and their consequent mutual contact will probably offer a much more efficient resistance, than can be made by the internal membrane though mechanically stronger.

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* Med. Ess. & Obs. published by a Society in Edinburgh, Vol. ii. p. 232.

† Page 69. ‡ Treatise on Hæmorrhage, p. 4. § Ibid. p. 3.

The formation of an aneurismal sac is a process, totally differing according even to common opinion, from mere distension. Where indeed strong exertion forms an aneurism in a previously sound vessel, the first effect must be that of rupture, or simple dilatation; but in the after stages of the disease from this cause, and in all the stages of that from the more common causes, the enlargement of the inclosing parts proceeds from changes in organization, which leave the extended sac generally of equal thickness with the sound artery, and often of greater. This process of gradual change, beginning for the most part in a thickened state of the internal membrane, has as little similarity to distension as the gradual enlargement of the uterus during gestation; and it is not much more difficult to conceive the increase of the muscular fibres in the one instance than in the other. If therefore the experiments relating to the dilatation of the dead vessel, were true indices of the phenomena of distension in the living animal, they would nevertheless be inapplicable to the present subject. But is it indeed true as a general rule, that muscular fibres cannot readily bear distension without rupture? We greatly doubt the fact. In parturition the muscular fibres of the os uteri, and of the vagina must frequently be elongated to six or eight times their natural length; yet they are not usually broken. In ascites the parietes of the abdomen are not unfrequently stretched to a great extent, even to the bursting of the integuments; yet here neither the thin flat, nor the thick straight muscles are usually torn asunder. In tympany the slender muscular covering of the intestines is often prodigiously enlarged, though seldom broken. The muscular coat of an artery is said by Jones to differ from other muscular structure in its elasticity, a quality which is hardly likely to add to its brittleness.

We state these objections, not because we wish to controvert the author's doctrine of aneurism; but because we are anxious to remove all petty props from that which may be able to stand without them. Whatever the actual observation of aneurisms may shew, by that, and that alone, are we to be guided: and we fear, that the subtlety and research displayed by an author in these minor arguments, may lead his readers to suspect a deficiency in this direct and only available evidence.

The testimony adduced on this important point, is not however either indistinct or scanty. The author details from his own observations and from the writings of others, copious histories in proof of his opinion. These details relate to aneurisms, as well external as internal, as well of the smaller as of the larger arteries. These details constitute one of the most instructive parts of the present volume. We neither wish them extended nor abridged. The opposing testimony we think is by no means of equal force; though it is obvious, that in this instance as in others which involve the proof of a negative, the most numerous testimonies on the one side must be insufficient to stand in opposition to a single clear testimony on the other. Mr. John Bell, as we have seen, asserts that the muscular structure can be more readily traced on the surface of a natural aneurism than of a sound artery. We wish that he had distinctly referred to those notes of dissections whence he drew the right to make this assertion. We know but one example which can be regarded as at all precise. Of the importance of this Professor Scarpa seems so thoroughly aware, that he has bestowed considerable time and ingenuity in endeavouring to obviate those conclusions to which it apparently leads.

We shall place these opposing testimonies before our readers in the words of the respective writers.

"On removing," says Scarpa, "the cellular pulpy sheath of the aorta, it is found smooth externally, like the artery; villous, cellular, and irregular interiorly, extending from the circumference of the tube of the artery over the neck and fundus of the aneurismal sac. This external covering or sheath of the artery actually appears to those who are not sufficiently skilled in such dissections, as if the artery were dilated under it to such a degree as to form the aneurism: and it has still more that appearance if the aneurism is very large and of long standing, since in this case the cellular sheath of the artery becomes unusually thick and pulpy; and because it adheres very firmly to the subjacent muscular coat of the artery, at the stricture or neck of the aneurismal sac. But even in these cases, as well as in those of recent and small aneurisms of the aorta, by employing care we may at last succeed in separating without laceration this cellular sheath from the tube of the artery, above and below the injury, and successively from the subjacent muscular coat as far as the neck or root of the aneurism. It is then clearly perceived that the muscular coat of the aorta does not pass beyond the partition which divides its tube from the entrance of the aneurismal sac, but terminates like a fringe, or in obtuse points at the edge of the rupture of the artery*."

What is here said of the aorta, we may observe, is afterwards applied to aneurisms in all other parts of the body.

The particulars of the case to which we have alluded, as published by Monro primus, are the following. A man forty-eight years old was affected with tumors in both hams, in the right groin, and about the middle of the right thigh, at the same time. That of the left ham was circumscribed, about the size of a large hen's egg, and had a strong pulsation. That in the right ham was large and diffuse, and pulsated. The swelling of the right groin was not larger than a small hen's egg, and that in the middle of the thigh not larger than a pigeon's. These were pulsatory and circumscribed. The diffuse tumour alone caused pain and swelling of the leg; and from this a fatal hemorrhage ensued. The tumours were sent from St. George's Hospital, by Dr. Donald Monro to his father, by whom and the present elder Monro they were dissected.

"The external loose cellular, and the cellulo-membranous being dissected away carefully, the circular fibrous, commonly called muscular coat was evidently seen continued on all the three smaller sacs in every part of them; but was thicker there than in the sound part of the artery. In the most enlarged parts of the sacs, an extraneous substance resembling a soft steatomatous matter, was intermixed with the muscular fibres. The cellular substance lining the inside of the muscular coat was considerably thicker than natural, and had much the same appearance of an extraneous substance filling its cells. The internal membrane of the artery adhered so firmly to these cells that it could not be separated, but seemed thicker than in a sound state. Though the circular fibres could be observed at the sides of the incision made into the fore part of the sac of the left ham; yet as the dissection was continued backwards towards the most depending part, these muscular fibres became less observable, and could not be traced. Whether this apparent defect of them here was owing to a much greater proportion of the extraneous substance above mentioned, or to their having been destroyed by the great distension, is difficult to determine. The internal cellular coat of the sac was considerably thicker than in smaller ones, but of the same texture. The most internal membrane was in a thickened adhering state. In the part of the great sac of the right ham which came to Edinburgh, no circular fibres could be seen, and the stricture was otherwise much the same as that now described of the back part of the sac in the left ham.*"

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* Page 78.

Such are the details of the contradictory evidence which has been given on this point. Our readers must judge by the future testimony of those who have the best opportunities to observe, to which credit may be due. We can by no means admit the validity of those objections which have been urged by Professor Scarpa against the testimony of this dissection by Monro. He seems to admit that the tumours in the hams were really aneurisms; but denies that the smaller ones were any thing more than steatomatous indurations. "These thickenings of the proper coats of the artery, instead of forming sacs or receptacles for the arterial blood, rather diminished the natural diameter of the artery†." From a very careful perusal of the case and subsequent observations, we cannot find any thing to authorize this assertion. On the contrary it appears to us very unlikely, that so accurate an anatomist as Monro, should call those "three small sacs," which were in fact tumours diminishing the capacity of an artery. In the observations subjoined by Dr. D. Monro to this case, we have even a positive assertion that "in the case of John Parker the sides of the cysts which remained entire, were nearly of the natural thickness of the arteries;" and that "in the one which had burst, the sides of the cyst were thicker and firmer‡." After such assertions our author cannot maintain his point without proving, not only what he has declared, that these cases were "mistaken for aneurism" by the Monros; but also that they have been wilfully misrepresented by them. The testimony of Monro, we must remark, on this side of the question is unusually strong, because it is that of one who had previously taught and published very sceptical opinions on this point. Influenced probably by Dr. Nicholls' researches in 1728, he seems to have taken every opportunity of examining minutely the appearances of aneurism after death; and a few years afterwards he published some opinions on the subject, of which we cite the following summary.

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* Ess. & Obs. Phys. & Lit. of Edin. Vol. iii.. p. 191. † Page 121.
‡ Ess. & Obs. Phys. & Lit. of Edin. Vol. iii. p. 254.

"From the whole we may see, that what authors call now-a-days a true aneurism, will very seldom be formed; which may be still further confirmed by mentioning the remote causes which are agreed on by all to occasion it for ordinary; these are wounds, bruises, straining, loud laughing, crying, &c. All such, you see, make a sudden violent effort on the arteries, and therefore do not rightly answer any of the suppositions we made of the manner this disease could possibly be brought on. And to establish what you see I argue for, of the true aneurism being a very rare disease, I perused a considerable number of notices of aneurisms, besides those mentioned by Dr. Friend; and could not find above two or three that were dissected, so much as alleged to have been true aneurisms; and there was not one where it is said that the aneurismal sac consisted of strong annular, muscular fibres, which however must be the true criterion whereby the true aneurism can be known, seeing blood extravasated in the tunica cellularia will have all the other symptoms, that are described as proper to the true aneurism*."

We presume not to give a decided opinion on this controversy; for the dissections of aneurisms which we have witnessed, by no means approached to that minuteness which alone can authorize decision. The body of evidence given by Scarpa from his own dissections and those of others, seems far to outweigh any opposing testimony with which we are acquainted, and as the subject was warmly agitated more than eighty years ago, it seems strange that direct counter-evidence has not since been copiously produced, if such evidence could be found. Yet we do not see how this case of Monro's can be fairly disposed of. We think it probable that the question will not soon be placed finally at rest. Supposing the opinion of our author to be true, it cannot easily be proved. In all instances of aneurism the changes of structure are great; and in those instances in which it has proceeded from previous disease in the arterial coats, they are such, we are convinced, as can by no means be traced by the knife of the anatomist. How can it be expected that in an aneurism which has continued for months or even years, and which has probably been preceded by a "steatomatous, earthy degeneration," we can detect an obvious solution of continuity in the inner coats, surrounded by the fringed extremities of the broken muscle? That a change of structure, or as Scarpa says, a "degeneration" occurs in this disease in the outer coat or sheath of the vessel, must be admitted, and we believe that in many cases it will be impossible to prove by dissection, that the two inner coats are not rather affected by this change than by decided and obvious rupture.

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* Med. Obs. and Enq. of Edin. Vol. ii. p. 242. This was first published in 1782. The Edition from which we quote is the 3d, published in 1747. The case of John Parker occurred in 1760.

The more important parts of the subject, the various modes of relief which have been practised, and the circumstances under which they are respectively useful, next occupy the author's attention. Till of late it has been usual with surgeons to suppose that in many or most examples of spontaneous cure, or cure by compression, the effect was produced by gradually strengthening the weakened vessel so as ultimately to enable it to resist that pressure from the blood to which it had previously yielded*. It is now however we believe a principle very generally admitted, that the cure of aneurism, whether spontaneous, by compression, or by ligature, can only be complete when the arterial canal is obliterated above and below the sac. This opinion is strongly stated, and copiously illustrated by Professor Scarpa. It certainly derives support from what he has shewn of the frequent, or as he says, the universal occurrence of rupture in this disease; yet it cannot be said to derive its chief support from this source. Modern authors who do not admit the universal truth of his doctrine, have insisted no less strenuously than himself on this general principle of cure†. Spontaneous cures of aneurism occur so rarely, that a surgeon cannot rationally defer his interference in expectation of their occurrence. When indeed the aneurismal tumour grows less and ceases to pulsate, and when at the same time those variations of temperature occur which commonly attend the change of circulation from the trunk to the anastomosing branches, a surgeon would be culpable were he actively to interfere. It is not easy to decide when compression is likely to be of use. If the limb be in a state of much irritation, this treatment may not only be insufferably painful, but may produce serious inflammation. If the tumour be so situated that it cannot be readily compressed; or if when compressed the vessel be not in a state prone to adhesive inflammation, bandages can be of no avail. Some of these circumstances cannot be ascertained previously to trial. Our practice nevertheless may be very safely guided by the following general rule: "The compression is contra-indicated whenever the popliteal aneurism is spontaneous, or not depending on a wound or extraordinary violent stretching of the artery; when the popliteal aneurism is of long standing and of prodigious size; when it is very hard; when it occasions acute pain and sympathetic fever; when it has produced considerable swelling of the leg and foot, with a diminution of their heat; or when the aneurismal sac is situated too low or too high in the ham*." The method of applying compression recommended by Scarpa, which is the same with that of Theden and Genga, seems to us unexceptionable: but on this head as our sentiments have been fully expressed in a former number, we shall now be silent.

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* See the work of Guatani, &c.

† Mr. John Bell among others.

That part of the present work which relates to the cure by ligature is obviously the most defective. The minute investigations of Dr. Jones have given a degree of precision to this subject, the want of which cannot reasonably be objected to any antecedent writer. Before his essay however such information had accrued from experience, as had led to practices totally different from those recommended by Scarpa; and we are therefore obliged to regard this part of his work as not only not beyond the information of his day, but as falling below it. The great danger of the operation for aneurism arises from secondary hæmorrhage; to obviate or remedy which, ligatures with various complications were employed. Experience however has long since decided that all such complications are injurious; and the best method in practice before Jones's essay appeared was certainly that revived by Abernethy, the division of the artery between two simple ligatures. By this method no great surface is separated from its vascular connexions, exposed, or irritated; and the ends of the divided vessel are allowed to retract within the support of the neighbouring muscles. Scarpa's mode is that of Mr. Hunter with the following modifications.

"Of all the steps of the operation the following points deserve particular attention. 1. The place of incision which I prefer, is in the upper part of the thigh or a little higher than the place where Mr. Hunter used to perform it, in order to avoid the necessity of removing too much from its situation or of turning back the sartorius muscle for bringing into view and tying the superficial femoral artery below it. 2. The manner of insulating the superficial femoral artery from the cellular substance with the point of the finger rather than with a cutting

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* Scarpa, p. 230.

ing instrument, in order to prevent in this way the division of any collateral branch; and the insulating the artery only in that space which is required for the application of two ligatures near to each other, and of a cylinder of linen corresponding exactly to the breadth of the finger or a little more. 3. The ligature by means of two waxed tapes of convenient breadth, placed behind and round the artery near to each other, with the interposition of a roll of linen of a cylindrical form between the artery and the knot. 4. The express omission of the ligature of reserve. 5. The giving the preference to the single rather than to the double or surgeons' knot. 6. The unremitting attention during the subsequent cure, that the lips of the wound do not approach too near, and still more that they do not adhere together before the ligatures and the roll of linen are expelled from the bottom of the wound, and till the bottom of the wound has risen nearly to a level with the integuments. 7. The timely incision or counter-opening in the case, although it is not frequent, of an abscess forming in the vicinity of the knee, along the course of the superficial femoral artery, occasioned by the portion of the cellular substance surrounding the artery passing into mortification*."

The chief deviations in this practice from that of the present day, consist in the introduction of a cylinder of linen, the maintenance of the continuity of the vessel, and the prevention of the wound from healing till the discharge of the ligatures. Of the substance introduced between the ligature and the vessel, we can by no means approve. We have indeed seen it practised, and with success, in a manner and with a view somewhat different from those of the present writer; but we object to it from comparative estimate of its success with that of Mr. Abernethy. This comparative estimate, according to the experience and information of Scarpa, is against the operation which we prefer, and under such impressions each party will of course be inclined to persevere in his own opinion unless the later experience of Dr. Jones may turn the scale. The obvious difficulty in this practice consists in the attempt to produce a certain degree of inflammation in the vessel, without producing too much. Scarpa maintains that in order to secure the closure of the canal it is merely sufficient to keep the sides of the artery in close and firm contact, without circular constriction; an object which he proposes to effect by a moderate compression of a cylindrical linen somewhat broader than the artery, by which means the vessel is tied when flat. This apparatus, we are satisfied, would produce too much general inflammation on the artery, with a consequent tendency to ulcerate. It has been shewn by Desault that whenever a ligature is firmly applied to an artery, its effect is to divide the internal coats; an effect which Dr. Thomson and Dr. Jones think essential to its success. Any ligature then, they argue, which can produce this division in the most simple manner will be the best. Mr. Wishart, the translator of the present work, affirms that he has repeatedly tied arteries in the manner of Scarpa, and invariably found the inner coats divided by it. We doubt that Mr. Wishart has in these instances used more force than was intended or used by his author, each of whose ligatures was two lines in breadth; though this is in fact unimportant to the argument. If it be necessary to the success of a ligature that it should divide the inner coats, then the ligature of Scarpa, if it do not divide them will do too little, and if it divide them it is more than is sufficient for the purpose. The details of Dr. Jones have sufficiently proved the superiority of a small, round, even cord; but whether one such applied according to his directions be preferable to two with intermediate division, must be left to be decided by that experience which we believe is rapidly accumulating. Where ligatures are applied so as to surround a piece of linen, it would we conceive be improper to attempt union by the first intention, for the presence of such an extraneous body would naturally impede the process; where however the more simple mode is adopted of tying the vessel with one small ligature we do not see the propriety of exposing the wound to suppuration.

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* Page 274.

A proposal is made by the author to adopt under certain circumstances a mode of operation to which he has never yet had recourse, and of the propriety of which we must confess that we see great reason to doubt. When an aneurism of the ham is of great bulk, of long standing, and when it seems likely to burst; when the patient is advanced in years, and has been worn with acute pain; when the leg is swollen, ædematous, and little sensible; and when there is reason to fear that the bones of the joint are spoiled; it is obvious that little can be hoped from ligatures, wheresoever applied. Under such circumstances it has been proposed to amputate below rather than above the knee, by which it has been thought that the risk of mortification to the stump would be less. To this proposed mode of operating, for it does not appear that the trial has been ever actually made, and indeed to every other Scarpa prefers that of amputating, or as he expresses it, disarticulating the leg at the knee, the femoral artery having been previously tied below the profunda. This preference has been given for the following reasons:

"In the first place because, as has been inculcated by Hildanus, and afterwards by Brasdor, and from what I have had repeated occasions to observe, disarticulations are much less subject to be followed by severe symptoms than amputations; in the second place because, in making the disarticulation of the knee, turning the patella upwards, dividing the lateral and crucial ligaments, and carrying the incision posteriorly downwards towards the calf of the leg, the aneurismal sac is evacuated in such a manner that the popliteal artery may be tied beyond it, as high up as the seat of the rupture requires. I would not dread much in this case the corroded state in which the posterior and inferior surfaces of the thigh bone are usually found; since this is properly speaking not a caries, but rather an excess, to say so, of absorption of the bony substance, occasioned by the violent pressure which the aneurismal sac and the clots contained in it have exercised on the bones; on which pressure being removed, there is every reason to believe that the bony substance will re-acquire enough of vitality at the place of the corrosion, to assume the process of granulation and adhesion with the soft parts which surround the stump."

We wish that the author had spoken more precisely as to the particulars of his experience in laying the cavities of large joints in a state of severe disease, freely and widely open. We have never witnessed any thing that can be regarded as at all analogous. We have seen indeed the cavities of the larger joints when sound exposed without any consequent evil. We have even known in one case the arm removed at the shoulder joint during the prevalence of hospital gangrene, yet this large wound healed readily and soundly, escaping the contagion with which smaller wounds were hourly and fatally seized; though this exemption might be attributed to the greater care bestowed on this case than on trivial ulcers. In all those instances however with which we have been acquainted, there has been not only a sound joint, but a large cushion of muscular flesh, and a flap of skin to fill and cover it. In the example before us, the joint is supposed to be greatly diseased; and whatever may be the nature of this disease of the bones it is probable that from the extensive destruction of the periosteum considerable exfoliation must occur, especially where no large muscular cushion or surface of skin exist, to save the wound from extensive suppuration. The proposal is doubtless made to avoid the evils of a great flesh wound; but if the large aneurismal sac be removed and the artery tied above the ham, a great flesh wound is inevitable; and if the vessel be tied in the fore part of the thigh and the sac left, we have not only the concurrence of two severe operations, but we have in the immediate neighbourhood of the wound of one of them a mass of stagnant blood on the eve of corruption, and a mass of half-gangrenous solids which had already threatened the destruction even of life. In such an operation we cannot see any probability of good; yet we cannot deny its possibility, and under such deplorable circumstances the prospect of possible relief may justify much boldness. In the amputation below the knee we see if possible still less hope of advantage.

The chapter which treats of femoral aneurism is the least interesting in the volume, since the author has never seen the effects of a ligature applied above the profunda; nor had extensive access to that information which has been recently accumulated on this subject. From the testimonies however of some surgical authors of credit he has no hesitation in advising the ligature of the femoral artery below the crural arch, though he does not appear to have conceived the possibility of tying the external iliac artery above it. He justly regards any attempt to cure aneurism in the thigh by compression as hopeless, since the artery cannot in any part of it be so fairly and steadily forced against the bone as to produce the adhesion of its sides. When such an aneurism occurs in the middle or lower part of the thigh, or within four inches of the crural arch, if it be circumscribed and unattended by any bad symptoms, he proposes to operate for it in the same manner as if it had been in the ham; if it be of considerable size, of long standing, and if it threaten to burst or mortify, he advises the old operation of opening the sac and tying the vessel above and below it. If the disease be seated higher up, he directs the femoral artery to be tied below the profunda, if possible, and if not, above it.

As we have already bestowed a great deal of time on what appears to us the most prominent parts of this volume, we shall pass over the remaining chapters with a short and general comment. They relate to the trachial aneurism which generally arises from a wound, and to the aneurismal varix which always does so. In these chapters is collected very copious and precise information from various quarters, as well as from the practice of the author; and we have upon the whole derived more unmixed satisfaction from them than from any others which the work contains. They are full without being crowded, and minute without descending into trivial and unimportant particulars.

Having taken so full a review of various parts of the work before us it may be thought superfluous to give a summary opinion of the whole. In doing so we shall at least be brief. The present volume is of very high value, for whatever may be thought of some of the speculative opinions and practical conclusions which it contains, yet it must be allowed to comprise a larger store of important information and rational opinions on the subject, than is any where else to be found collectively. Let it not be thought that because we have spoken of its merits chiefly in general terms, and have dwelt on what we think its defects in detail, that we are influenced by any paltry or malignant desire to lessen the high reputation of this great surgeon. His work is or soon will be in the hands of all the reading part of the profession in the island; and we have thought therefore, that we could render a more acceptable service to the public by pointing out what we think defects, which in many instances might be overlooked, than by enlarging on those prominent merits with which no man can fail to be impressed. In works of minor importance not likely to obtain so extensive a circulation, we deem it more useful to specify any valuable matter contained in it, and to pass over with a general notice errors which do not come to us recommended by the imposing sanction of authority. Besides the merits of Scarpa's work do not consist so much in particular trains of reasoning, or dogmas of practice which could be extracted or set apart for observation, as in the abundant knowledge which is brought to bear on every point of his subject, and in the sound sense and discretion with which the whole is cemented. From the three works which we have seen from Professor Scarpa we judge him to possess great zeal, indefatigable research, and a strong mind. That he has errors all will be willing to allow, though they may differ widely in regard to the particular examples; yet all we believe will be equally ready to admit that his errors are neither gross nor numerous, and that his works are marked by the distinct and genuine characters of temper, knowledge, and understanding.

The translator, we think, has performed his task, not only creditably, but ably. His language, with the exception of some scottish idioms,* possesses the first qualities which the language of science can possess; it is correct, simple, and clear. His own observations are seldom if ever original, but they are pertinent and judicious; and the quotations and cases which he has given, add in no small degree to the illustration and support of his author.

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* Particularly the uncouth phrase "too long of coming away," &c. which repeatedly recurs.

Art. III. *Anniversary Oration, delivered March* 8, 1808, *before the Medical Society of London, on the general Structure and Physiology of Plants, compared with those of Animals, and the mutual Convertibility of their Organic Elements. Published at the Unanimous Request of the Society.* By John Mason Good, F. R. S. Senior Secretary to the Medical Society. London; Longman and Co. Paternoster Row, pp. 56. 1808.

THOSE who are accustomed to attend debating societies must often have observed, that the same remarks which appeared striking when delivered in public, possessed no ingenuity, no novelty, nothing particularly worthy of attention when subjected to the cool revision of solitary reflexion. The Brunonian principle, that a stimulus always acts strongly in proportion to the weakness of that which immediately preceded it, is so extensively applicable to mind as well as to body, that it is impossible to ascertain the accuracy of applause without knowing what was acting on the mind immediately before it was uttered. We enter the society, we sit down upon the benches, we listen to some hesitating, muddle-pated, thick tongued orator, who is luckier than his hearers if he understands what he says. Speech follows speech, each vieing with its predecessor for the palm of dulness and incomprehensibility, till our mouths yawn and our eye-lids grow heavy. From this state of direct debility of the mind we are suddenly roused by an abler disputer. He is fluent and clear, he argues to the point, and conducts the disquisition in such a manner as to recal the attention of his audience, which had been drawn hither and thither by the wandering and driftless addresses of the former speakers. After an uninterrupted succession of obscure speakers, it is pleasant to hear a clear one even if he possesses no other quality of an orator. We listen to him with pleasure, we see him set down with regret, and we go away with a high sense of the value of his observations; but the eye which has been kept in darkness is dazzled by candle light; and of the many speeches which we hear with pleasure, how few leave upon the memory any valuable ideas. The striking speeches, and the anniversary orations of our debating clubs may afford delight during their delivery, but why print them? they lie in heaps upon our book stalls, and serve only to remind the passer by of the waste of paper and the transientness of reputation.

Our readers will easily guess the feelings with which we took up the Anniversary Oration of Mr. Mason Good, and conclude that these feelings were not altered by the declaration, that it was prepared during the bits and scraps of time left out of little more than a week, by the engrossing occupations of a London apothecary. We are sorry to say that our forebodings were verified by a perusal of the pamphlet. The author is well informed in natural history, and has collected together a number of facts, many of which are quite familiar, to prove, 1st. That plants like animals are propagated by eggs. 2d. That they are very similar in their vascular anatomy. 3d. That they are impregnated by sexual connexion. 4th. That they are sometimes propagated by slips. 5th. That their vessels secrete different substances from the same circulating fluid. 6th. That they are subject to similar diseases. 7th. That they may be divided into locomotive and fixed. 8th. That they are capable of retaining their natural heat in great variations of temperature. 9th. That they may be divided into terrestrial, aquatic, and aerial. 10th. That both contain examples of amphibiæ; and 11th. That both can live only in oxygen. After having endeavoured to prove these statements, sometimes in a very far-fetched way, he goes on to state, that vegetable matter is converted into animal during the process of assimilation by the abstraction of oxygen and carbon, and the addition of azote; and that animal matter is converted into vegetable by the contrary process. We might detain our readers by many parts of this pamphlet which admit of animadversion, but we think that the pages of our Review may be better occupied. We have said that some of the author's proofs are far fetched; let the following serve as a specimen.

"Animals are occasionally divided into the two classes of locomotive or migratory, and fixed or permanent; vegetables may partake of a similar classification. Unquestionably the greater number of animals are of the former section, yet in every order of worms we meet with some instances that naturally appertain to the latter, while almost every genus and species of the zoophytic order, its millepores, madrepores, tubipores, gorgonias, isises, corallines, and sponges, can only be included under it. Plants, on the contrary, are for the most part stationary, yet there are many that are fairly entitled to be regarded as locomotive or migratory. The natural order *senticosæ*, the *icosandria polygynia* of the sexual system, offers us a variety of instances, of which the *fragaria* or strawberry may be selected as a familiar example. The palmate, the testicular, and the premorse rooted afford us similar proofs:—many of these grow from a new bulb, or knob, or radicle, while the old root, of whatever description it may be, dies away; in consequence of which, we can only conclude that the vital principle of the plant has quitted an old, dilapidated, and ruinous mansion, to take possession of a new one. Insomuch that were a person, on the point of travelling to the East Indies, to plant the root of an orchis*, or a scabious†, in a particular spot in his garden, and to search for it in the same spot on his return home, he would be in no small degree disappointed; and if he were to remain abroad long, he must carry his pursuit to half an acre's distance, for thus far would some of these roots perhaps have travelled in a few years."

In a debating club, Mr. Mason Good would be called "an ingenious gentleman" for this piece of argument. If the original buildings of a town had been abandoned and gone to ruin, and new ones had been erected at a short distance bearing the old name, we suppose our author, according to the above reasoning, would seriously and literally say that the town had taken a walk.

We must take leave of Mr. Mason Good. We have not detained our readers long, but considering the value of the pamphlet, we have detained them long enough. Mr. Mason Good is well known as a man of sound sense and extraordinary acquirements, and we regret that he has been persuaded, even by the unanimous request of the London Medical Society, to publish any thing so little worthy of him. He should remember, what we are surprised so few of the scribbling race are aware of, that the permanent reputation of an author depends, not on the number but on the merit of his publications, and that although a merely fruitful pen may give him notoriety, it will fail to give him fame. We consider the present publication to belong to that numerous class which had better never have appeared. The field of literature is overstocked with vegetation, and it is our firm determination, as it is our bounden duty, to root up all the foul weeds and useless vegetables which come within our reach.

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* Orchis morio, or latifolia. † Scabiosa succisa, or devil's bit.

Art. IV. *The Principles of Surgery, Volume Third, containing a Series of Cases, calculated to illustrate chiefly the Doctrine of Tumors and other irregular parts of Surgery, and to instruct the young Surgeon how to form his prognostics and to plan his operations.* By John Bell, Surgeon. 4to. pp. 298. London: Longman, Hurst, Rees, and Orme, and Cadell and Davies, 1808.

WE have long thought a plain and faithful delineation of the external characters and internal structure of tumours, to which the anatomist and the artist should equally contribute, to be a grand desideratum in the science and practice of surgery. Tumours, from their obscurity and varieties, form a branch of surgery, nearly approaching the mysterious and multiform character of medical diseases. The symptoms of stone and rupture, the signs of luxation and fracture, the characters of wounds and ulcers are so familiar and intelligible to the practical surgeon, that the fact of their existence is sufficient to decide the mode of their treatment; but in a vast proportion of cases, it is not so with tumours. We are aware that many and not inconsiderable difficulties present themselves, in every attempt to illustrate the relation between internal and external arrangement. For one, we may instance the existence of such signs or marks as are typical of general and not peculiar alterations, of incidental and not original changes. Thus the turgescence of the superficial veins cannot be said to denote a *specific* change, as it belongs to all textures of preternatural density; and the prominent marks of colour, figure, weight, and fluctuation, not to dwell on the unceasing varieties of degree in which they exist, can be considered only as marks of general not specific distinctions.

Medical pathology, derived from an examination of the changes of internal structure, illustrates the relation between parts and symptoms: surgical pathology strictly speaking, confines itself to the relation between the visible and concealed organization of the part. The difficulties of the surgical pathologist are therefore greater than those of the medical, from the impossibility of discovering functional changes in parts of little subservience to the vital economy. A bone, vessel, or lymphatic gland is concluded to be sane until it palpably assumes an altered texture, because the sympathy of the constitution is not perceptibly roused by the derangement of such a part. But the functional remarks on the fatal increase which they are suffered to attain from the patients' imprudence or unconsciousness of danger, and the irresolution or timidity of the surgeon. We pass over the many cant and weatherbeaten admonitions which habitually escape a *cidevant* lecturer addressing his pupils, to present our readers with a very characteristic extract from this discourse.

"The false taste in science, in practical science especially most false, of heaping wonders upon wonders, and esteeming nothing worthy of being narrated but what passes the bounds of Nature's wonted operations, has prevailed much and long.—This can never be any part of my ambition: neither shall I affect to tell with precision how a tumour shall be defined, whether as a part newly formed and not natural, or as the morbid increase and growth of some natural organ; nor shall I seek to arrange tumours with scholastic precision, according to their species and genera, filling up the outlines of a theory, or the gaps of a philosophical system, with insipid reasonings, or with tales extracted from books. My opinions on nutrition, sound or morbid, and on the varieties produced in that function by the degrees and modifications of vascular action, I need not conceal, but shall never obtrude upon you; it will be easy while narrating facts, to insinuate without illustrating such opinions."

This paragraph is we think a *quietus,* in full of all demand, for the lovers of system. "*Not to obtrude, though at the same time not to conceal,*" and "*to insinuate without illustrating,*" must be pretty well understood indirectly to convey what our author would be very reluctant candidly to confess, viz. that he has in truth no accurate ideas on the subject. To support this shifting and shuffling, he ingeniously affects to ridicule every attempt to pathologize, and is of course silent upon the analogies and relations of tumours in their seats, growth, and characters; the only method of observation which we contend can supply that deficiency of elementary works which he is so ready to deplore, or reflect any useful light upon the *doctrine* of tumors. Which of Mr. John Bell's readers will doubt, that he would rather be excused the task of defining with precision, and arranging with accuracy, what it requires time as well as diligence, and temper as well as talent to understand? But to scout definition and arrangement, because, like every thing excellent in philosophy and literature, they have been subject to the abuse of dabblers and sciolists, is to leave us satisfactory proof that Mr. Bell's power of deciding upon their utility is the result of a very superficial acquaintance with their employment, and fully to confirm our foregone suspicion, that they are instruments for the management of which he is notoriously disqualified. From this single sentence the tenor of this performance may be inferred. It is not that conciliatory species of self-detraction, with which the *exordia* of great writers and orators abound, setting forth the magnitude of their undertaking, and the consciousness of their inadequacy: No! This is a conceit too common place for the genius of Mr. Bell. By a sarcastic contempt of others he endeavours to blind the reader to his own inability, and then politely frames a very *modest* request to be permitted (disregarding vulgar rules and formalities) to stray wherever his fancy leads him; to wander, like a bee in a parterre of flowers, *among the irregular parts of surgery.*

"It is a department of the science which requires to be taught in the form of particular lessons, practical not speculative; founded on actual observation; where cases of various complexions tend to illustrate every variety of danger. The operations of lithotomy, trepan, and amputation are easily performed, and are performed in one unvaried way. The surgeon should know how to resolve a tumor, or to excite it to suppuration; how evacuate the matter, and obliterate the sac; when to suffer the mild and harmless to grow, and how to extirpate at every risk that which is prone to ulceration or cancer. Every tumor is a subject of new and anxious consultation, and every operation of this irregular nature, demands a knowledge of blood-vessels and nerves not easily remembered, and perhaps no where truly described.

"Need I remind you what torture a skilful surgeon may prevent, what misfortunes ignorance may cause? Every day we see the surgeon mistaking blood for matter,—flesh for bone, and tumors, malignant in their nature, for indolent and harmless swellings of the glands: by tormenting a simple tumor it may become malignant,—by neglecting a moveable tumor it may become fixed,—by allowing a small and seemingly harmless tumor to grow, it may wax to so great a size, and acquire such intimate connexions with the more important vessels and nerves as to defy all kinds of surgery. A tumor of the jaws, the throat, or the nostrils, of which the surgeon in its first beginning speaks too lightly, grows in process of time to such a size that it compresses the throat, fills the passages of the nostrils, and, by its growth and pressure, renders the bones carious, and affects the brain. And thus it comes to pass, that, when the patient returns after some months of absence to crave advice, and still more earnestly than at first to intreat for help, his approaching death is manifest and inevitable."

Every department of our science requires particular lessons; but is not the value of this form of teaching indescribably augmented by making them illustrative of general propositions?

How is the surgeon to acquire that discriminative knowledge, the possession of which is represented to be so important, the absence so alarming? Not by a croud of straggling incidents, or a volume of insulated observations, but by a careful comparison and judicious arrangement.

Discourse II. *On the unlimited growth of Tumors.* After informing us that it is sufficient to know that nutrition and secretion exist, without inquiring by what means; that we might as well pretend to investigate the process of generation as that of growth, that rivers, trees, and animals are not less individually the same rivers, trees, and animals, because their integral particles are incessantly changing; our author concludes that betwixt healthy nutrition and that which constitutes disease, no difference exists except in degree. Then follow definitions or rather descriptions of nutrition and of tumor.

"Tumour is an excessive secretion and assimilation of new particles, exceeding the necessities, and transcending the natural limits of the part; an augmentation of bulk, without a change of texture; for it is an addition, particle by particle, of bone to bone, skin to skin, gland to gland, muscular flesh to muscular flesh, &c.—If skin is irritated, bone fractured, a gland, &c. bruised and injured, the vessels of such part are excited, but not in such degree as to destroy its texture, *not* so as to produce inflammation, effusion, and ulceration or gangrene, but so much only as to administer nourishment more rapidly and profusely: the part thus affected grows but is unchanged: the tumor is skin, bone, gland or muscular flesh unaltered: if fat is secreted more profusely, the tumor is of fat; if any natural secretion be increased and confined, as in glands or bursæ, the tumor is fluid and sacculated.—It seems to me that every tumor is a mere accretion of nutritious particles in skin, bone, gland, or muscle, according to the nature of the part: tumor is, in short, either an increased nutrition, or an increased secretion, modified indeed, in its form and character, by many changes produced by occasional inflammation or ulceration.

"There are but two conceptions we can form of the nature of tumor; either that something specific in the matter, or something peculiar in the vascular action of a part produces a tumor, firm or soft, of fat, of cartilage, of bone, or of flesh, according to that specific action; or that the properties of the vessels, which are by violence or disease, by a bruise, fracture, cold, heat, or other foreign cause, thrown into excited action, determine the nature of the tumor, or, in other terms, of the secretion, fluid, or solid of which it is formed."

An excessive secretion and assimulation of new particles,—an augmentation of bulk without a change of texture,—an addition, particle by particle, of bone to bone, skin to skin, muscular flesh to muscular flesh, &c.—a mere accretion of nutritious particles,—an increased nutrition or an increased secretion. Such is tumor.

What a confused jargon of words to define one poor solitary idea! What *an augmentation of bulk without a change of texture!* But according to this notion, how is it possible to say with precision where nutrition ends and tumor begins. By nutrition we mean a healthy process of the economy, by tumor a disease; if tumor is only nutrition in excess, then disease is health in excess, and debility is not the want, but the superabundance of power*.

But does not this excess of action, which produces tumor, differ most materially in its nature from the action of nutrition? Is it not that peculiar and modified action (of whatever kind) which produces all the diversities and varieties of animal productions, healthy as well as diseased? In short, can a tumor be formed by an action of vessels so slightly augmented as not to constitute inflammation? Do we not daily see examples of inflammatory action without a destruction of texture? And does not the presence of inflammation, chronic or acute, serve as the diagnostic mark between natural and preternatural growths?

It is possible that inflammation may, and often does for a short time elude the cognizance of our senses; but from the moment in which unnatural growths or any other of its phenomena become manifest, it is reasonable to assume the existence of a morbific action: that every morbid structure is susceptible of nutrition, is unquestionable, but this its own peculiar nourishment is unconnected with the process which preserves the animal from decay. We agree with Mr. Bell, and we believe with most others who have bestowed a thought upon the subject, that two conceptions may be formed of the nature of *tumor*, viz. either the vascular action is specific or peculiar, or it is simply increased. But why admit one of these conceptions and reject the other. We have no hesitation in receiving both. Though nature is an admirable economist of causes, yet if we consider the infinite varieties of tumor, we think it will hardly be granting her too much. When action is simply increased as in simple inflammation, the natural properties of the vessels determine the nature of the tumor. But although in the plurality of cases, skin secretes skin, bone is added to bone, and gland to gland; yet it is by no means unusual to see horny growths from the cutis, fatty growths from serous membranes, permanently soft effusions from bone, and osseous depositions among muscular and glandular parts; nay, we have heard of a tumor in the region of the kidney containing teeth and hair, and what is still more wonderful, of a descending series of infants in each others bellies.*

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* We can see no reason more logical for admitting an excess of action to be healthy than its contrary. We might as rationally say that a watch goes well, because it gains an hour in the four and twenty, or praise a man for his punctuality who arrives *sooner* than he is expected.

Mr. Bell next proceeds to unfold with appropriate solemnity the very novel fact, that the effect of exciting the vessels of a part to action, is to increase the growth of that part. This is the remaining sum and substance of this discourse. Lest we should forget them, he drums us through the tedious catalogue of stale examples; of porters' legs, and sailors' loins, and carpenters' arms; of the impregnated uterus, the secreting breast, the dropsical belly, the dilated vein, the aneurismal artery, the hernial sac, and the old neglected hydrocele; and these examples of the effects of exercise, pressure, extension, and distension upon the animal body, (which have the reverse effect upon the inorganic) are illustrated by cases of enormous elongations of skin, exotoses, glandular swellings of the neck and face, and lastly, we have a case of frost-bite terminating in gangrene, to shew the excess or exasperation of vascular action from over excitement, and its tendency to periodical returns. These cases are again illustrated by frightfully animated portraits of the pitiable wretches who were the subjects of such deformities. Amplification and redundancy are such notable and leading features in all our author's productions, that the actual sum of his originalities is very quickly estimated. We will venture to assert, that in the whole mushroom tribe of students in surgery, there is scarce a man who has not these "saws and instances" by heart. They are as familiar to him as his alphabet, and he has heard the one as frequently re-echoed in the class-room, as the other in the nursery. But then he has heard them unaccompanied with boisterous abuse of the old theorists, undiverted by entertaining fables, and stated in all the dullness of pedagogic conciseness. Here he has them in a popular dress, enlivened by dramatic interludes, with much new and appropriate scenery, machinery, and decoration; and withal so well supported by a vigilant attention to stage effect, and what is technically termed *bye-play*,† that his senses are almost tricked into a belief that these things are so many real additions to his useful knowledge.

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* See Baillie's Morbid Anatomy, and Abernethy's Classification of Tumors, p. 10.

† *Examples.* The ridiculously minute and superfluous incidents used in *case-making*, and the introduction of whole length figures to shew an enlarged joint!

Discourse III. *On tumors of the bones.* After a long, rambling, irrelevant declamation, in which Mr. Bell gravely confirms the opinion of Celsus, "that diseases are not cured by eloquence," (a truism which it well becomes him ever to bear in mind) we meet with the following very curious exemplification of that statement which formed so much of the burthen of the last discourse. It is an account of the formation of a complicated exostosis. "From the *periosteum* is secreted *bone*, from the *marrow*, this *steatomatous and solid fat*,—to the *tendinous* and *aponeurotic* parts we can distinctly trace the *cartilaginous* secretion."

Several remarkable histories and drawings of bony tumors of the fingers, hand, wrist, and arm, to which Severinus, his correspondent Larchæus, and M. Mery are contributors; and a story of a barbarously ignorant surgeon who operated on a wrist tumor, where the sac consisted of the *supinator quadratus* muscle, (with which muscle by the way we have not the pleasure to be acquainted) are the contents of this discourse. One of the most prominent cases is that of a "fine, lusty, thriving creature," whose father writing to one of the author's abilities and practice proses learnedly on the cause of the complaint, (scrofulous nodosity of the finger joints,) and hesitates between foul milk and a propensity to crawl on hands and knees at six months old. To this application are returned two very pompous letters of consultation;* and the reader's curiosity is additionally gratified by a striking likeness of the "lusty creature" in his broad ruffled shirt, reclining against a fluted column with an elegant vase at his elbow, and stretching his ugly, misshapen claws upon a table. Mingled with some mysterious cant about the dispensations of providence and what not, the father (some Cumberland squire,) is referred for analogous cases to the Academy of Sciences, Vanderwiel, and divers moth-eaten authorities; and among other matters sagely told that the disease has been named the "*atheroma nodosa.*"†

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* Signed Alex. Monro, J. Bell.

† In a very voluminous note the treatment is laid down with all the laboured and minute precision of an old maiden aunt. "If the bowels instead of the mouth are affected by mercurials, give immediately *eight* drops of laudanum in a little sugar and water, put him to bed for an hour, and *he will feel easy and happy*. Should the *tortures* of his bowels continue, he is to take *twelve* drops, and a *glass and a half*

Discourse IV. *On tumors of the nostrils, gums, and throat.* Mr. Bell is stronger than usual in his remarks on the nature of polypi.

"In writings of systematic authors, all seems simple and harmless, and the methods of cure are trifling and temporizing. In the works of original authors, the methods are rude, violent, and unrelenting: rather than not unroot the tumor, they would burst up the cells of the face, and destroy all the bones; and they deliberately debate these questions, whether we should not split up the nostrils? trepan the antrum Highmorianum? and dig away the spongy bones? rather than fail to reach the roots of the tumor. Whether it be not allowable to perform bronchotomy, and by opening the trachea procure free breathing for the patient, while these more cruel operations are performing? this is a text, these are discrepancies, on which it becomes us to comment. Those who have heard and repeated the saying, that polypus is in general a mild disease, have of course believed that these are cases of peculiar malignity; that there are certain polypi which the surgeon need not disturb, and should not tamper with, and others which he assuredly cannot cure. But were this conviction ever brought home to the mind of the surgeon, then should his good offices be at an end; surely we must not in any case pretend to meddle with a disease, which, if mild in its species, we should not irritate; which, if malignant, we cannot cure? But *I* have some experience in this line of practice, and some judgment in this department of reading; and have strong suspicions of something wrong, if not unfaithful, in these opposite representations. Polypus is never mild, nor ever malignant; time, and the natural growth of the tumor, and the pressure it occasions within the soft and bony cells of the nostrils and jaws, must bring every polypus to one invariable form, in its last and fatal stage."

"I have traced, as simply as I could describe, with any regard to truth, the phænomena of this disease, from the stage reputed harmless and mild, to that reputed malignant. If, indeed, horrid symptoms could establish the fact of malignity, that there is not to be found in all nosology a more malignant disease than this: but aneurism, though it destroys the thigh-bone, the sternum, or the cranium, is not accounted malignant; neither is polypus malignant, though it destroys the cells of the face, and penetrates even through the æthmoid bone, to the brain. These consequences, as I shall infer from undeniable proofs, result merely from pressure."

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of strong Port wine negus. It is important further to know, that he is to have *bread and milk* for breakfast and supper, and when his mouth is sore, *rice puddings, custard, flour pudding, panada with sugar and wine,* (the quantities are unfortunately omitted,) or whatever Mrs. ———'s better judgment in these matters may suggest." The total of this gossips' story occupies no fewer than *eight* closely printed pages.

The speculative distinctions of the disease raised upon the authority of Mr. Pott, are severely but not injudiciously treated, and we entirely agree with Mr. Bell in thinking these distinctions not less dangerous than nugatory. "For while the doctrine of benignity and malignity is acknowledged, while the surgeon pronounces every incipient polypus mild, only because it is small and as yet harmless, and every polypus malignant which has attained a conspicuous size and more solid texture, and begins to affect the bones; while he neglects the beginnings and shuns all concern in the lamentable conclusion of the disease, no patient's life is safe."

We are disposed also to concur in our author's testimony of the general futility of noosing polypi, and of the palpable imperfection attending the attempt to remove them in an advanced stage by the forceps. Mr. Bell thinks it worth the while to refute anatomically the vulgar notion, (which has crept into systematic works,) that polypi arise from picking the nose. Polypus, he says, is seldom solitary; in shape long and bulbous, being connected to the membrane of the nose by a small peduncle, or stalk; the extremity alone, and this not always, is tangible by the finger."

"The anterior and posterior chambers of the nostril are separated from each other by a narrow slit, which the finger can never pass; that opening is somewhat of the carved form of the slit in the sounding board of a violin, and the intrusion of the anterior point of the spongy bone, which is the point that encounters the finger when thrust into the nostril, gives it this peculiar shape. There is a little opening above, and another below this projecting point of the spongy bone; through these the heads of the polypus project; one generally fills the opening above the spongy bone, another polypus usually fills the space below; there they hang pendulous, and are forced sometimes through this opening by the breath pushing them down very low."

This narrow slit or cleft of the internal nostril, formed by the projection of the spongy bone, is almost as effectual a prevention we should think to the passage of an instrument as to that of the finger; but if the bulbous head of the polypus can be passed, and the ligature affixed to its root, where is the objection to the reapplication of the ligature three or four times, or even three or four times four, if the number of polypi require it? The tube of Levret, and various similar devices projected for the removal of polypi by ligature, assumed as their leading principle the prevention of hemorrhage. This in the small state of the tumor is never alarming, and if it were troublesome, might be instantly suppressed by drawing a plug from the throat into the nostril. But Mr. Bell has never seen the operation succeed, whether submitted to dexterous or awkward hands, and has himself always miscarried in attempting "to catch a nasal polypus" in a noose; and this, though he has planned his little operations so "cunningly" that he imagined it impossible he should fail.

The following is certainly a plausible piece of reasoning:

"If I am correct in ascribing this difficulty to the direction in which the nasal polypus hangs, being the same with that in which the ligature must be drawn, my reasoning will be confirmed, by the converse of the proposition being true, viz. that guttural polypi, those which, in place of occupying the nostrils, pass backwards into the throat, are easily noosed. This is an operation to the happy success of which I can speak with confidence as perfect, as my conviction is, that the operation just described never can succeed, or very rarely. When the polypus is single, or when one polypus has arrived at such a size, as to render whatever others may be behind it trivial; when the tumor, after having long filled the nostril, projects from the posterior opening of the nostrils into the fauces, depresses the palate, hinders the swallowing as well as the breathing, and is both seen, upon depressing the tongue, and felt upon passing the fingers deep into the throat, firm, hard, and bulky: at this stage of its growth, when the surgeon most fears to grapple with such a tumour, it is in truth the most manageable! it may be extracted with safety; its root may be cut across by passing a curved knife along the nostril, yet not without difficulty; and if there be an internal tumor which admits of extirpation by ligature, this is it. It is the only case in which I can with perfect confidence promise to apply the noose, and where the tumor is thus visible in the fauces, the ligature must be passed through the corresponding nostril, hooked out from the fauces with a hook, or catched with the forceps, brought through the mouth beyond the teeth and lips, spread out upon the fingers, and by the help of the fingers (pushed deep into the throat) passed over the bulbous part of the tumor, and then the wire being pulled back through the nostril, it slides up to the root of the polypus, or near it, or may be placed pretty correctly by a little help. Now when thus drawn, the course of the ligature is transverse to the direction of the tumor, and is perfectly effectual in its operation; for the good effects of a ligature, thus applied, I would willingly be responsible, having so very often performed it with unvaried success."

As far as our experience goes, the three following stages of polypus are very chaste and characteristic sketches.

1. "In its early stage polypus has invariably that character which is usually denominated mild: it is small, moveable, pale, colourless, and has not as yet begun to affect the adjacent parts by the pressure; there is a watering of the eyes, sneezing, altered voice, and interrupted breathing, but as yet no pain, nor any fœtid ichor distilling

from the nose: from the smallness and pendulous direction of such polypus, to noose it is difficult; and from the numbers of smaller polypi which usually lurk one behind another, the perfect extirpation of all of them by applying the noose is nearly impossible. The happiest and most successful process is to extract the body or bulky part of such polypi with forceps, and to destroy their roots with caustic.

2. "In its next stage, the polypus grows to a great bulk, not only fills the nostril, but is visible in the throat, the voice is entirely changed, for not a breath of air reaches the bony cells, nor passes through the nostrils: the hearing is greatly affected; if you introduce your fingers deep into the throat, you feel a tumor so bulky, as at once to depress the soft palate, and compress entirely the mouth of one or both Eustachian tubes: the face is swelled and unsightly, the nose inclined to one side, blood begins occasionally to flow, and the matter distilling from the nostrils and throat begins to be fœtid; the pressure is now universal and begins to affect the bones, and the disease borders on that stage which is, I fear, incurable.

3. "In its third and last stage, when the passages of the nostrils and throat have been long obstructed, and the face much deformed; when the patient has long endured the rending head-aches, and pains proceeding from the distension; when the surgeon can distinguish, by pressing with his fingers, that the cheek-bones are softened, and the nasal bones become moveable, and fœtor and hemorrhages intimate the caries within; when the integuments of the face are puffy, the skin reddened, or livid, over the root of the nose, and the teeth loosened; when the stupor from pressure on the brain, and the chilliness from want of nourishment and loss of blood are great and continual, the disease is declining into its last stage, which we can hardly palliate, and cannot cure. This is the stage of the disease reputed cancerous, and operations undertaken in these circumstances, and performed, as I have seen them with rudeness, inflame the brain, so that the patient presently sinks into absolute stupor, and dies."

Discourse V. *On the various operations practicable in the several stages of polypus.* When the tumor is small, Mr. Bell lays hold of the bulb with his forceps, intending to nip it at the root where it is most slender, and this intention is frequently answered. If the bulb recedes before the forceps, then has the operator a better chance of reaching the neck. Where he does not succeed in perfectly clearing the nostril, he introduces a small scythe-shaped knife vertically, and when he has fairly carried it into the posterior opening over the palate, and feels the roots of one or more polypi, he turns the edge towards them, and cuts alternately from right to left, and with premeditation in that direction in which from the breathing and other signs he supposes the polypus to hang. The convex edge of the knife, for convenience sake, should be made cutting. When all is done which knife and forceps can do, he has recourse to the caustic; indifferent as to what parts he destroys, if he but destroys the disease. The portcrayon conveying a pencil of caustic he knows would be "extremely dangerous;" which, if it be properly used, is much more than we know. He therefore spreads a caustic paste upon lint, as if to make an issue, and pushes it up on the end of a director through the nostril to the part he desires to burn. To prevent excoriation, a stripe of sheep leather is conveyed high into the nostril, and laid smooth along the surface. The caustic piece of charpie is supported by cramming the nostril with dossils, which are *rammed* hard in at each dressing. The lint never falls into the throat, however it may approach the verge of the posterior opening. The mucus is picked away and the nostril washed, (sometimes with barley-water, or oxymel,) at each dressing, which process is repeated every second or third day for a whole month; and from the quick regeneration of the disease, or the appearance of a second tumor, excision with the knife is sometimes required a second time. This is we think, to say the least of it, a very clumsy expedient; and with what consistency Mr. Bell condemns the ligature because three or four polypi may require as many ligatures, and approves the caustic which is to be applied every third day for a month after the pain of excision has been endured, we are wholly unable to conjecture.

In treating of the operation adapted to the second stage, our author freely quotes the details of the rude and for the most part ineffectual practice of Celsus, Richter, Morand, Meekren, Abbé Ceers, Manné, &c. and of that more recently in vogue among the best French surgeons, of slitting the nostril and velum palati from end to end. We have the narrative of a curious case of total adhesion of the soft palate to the back of the pharynx, closing the posterior opening of the nostrils. The author is, "*comme a l'ordinaire,*" rather long-winded in his "advice" upon the occasion, but sufficiently sound. It requires little surgical sagacity to see that the proposed remedy, viz. the artificial division of the adhering parts, would be but a sorry exchange for the disease.

The author's mode of operating on polypi which fill the nose and depress the palate, whose bulk is in his opinion a circumstance favourable to the operation of the ligature, consists, 1st. In passing a loop, or noose of silver wire with the left hand through the affected nostril. 2d. Hooking it out through the mouth. 3d. In spreading the loop upon the fingers of the right hand, gathered in form of a cone, and by a sudden and decisive effort carrying it over the biggest and most pendulous part of the polypus, while the wire is gently retracted with the left hand. 4th. In hitching up the ligature close to the root by means of a small curved canula passed alternately over each end of the wire as it projects from the nostril, and ultimately secured over both ends.

"Fifthly, *Of the effects of the ligature on the tumor, and of the time of its separation.*—When you first draw the noose, the stricture is followed with extreme pain, the eyes fill with water, the patient cries out and retracts his head, and violent sneezing follows; during the whole of the first and second day, the pain is like that of severe tooth-ach, and, upon tapping with the finger upon the catheter or probe, you find it firm. On the third day, a thin and bloody serum begins to distil from the noose, and continues to flow in great profusion; the probe or catheter is blackened by the putrid taint of this serum; the polypus, if any part of it project so as to be felt, is perceived to be flaccid, the breath begins to pass through the nostril; and the patient, who had felt his fauces choaked with the polypus, and was deaf from its pressure, now swallows easily, and hears very acutely, because the tumor begins to shrink.

"These are the first auspicious signs of the fading of the polypus: the fœtor of the matter increases on the fourth and fifth days, the probes and silver wire are still more blackened, the wire manifestly has become looser from the shrinking of the tumor, the catheter now shakes from side to side, and that it may completely destroy the polypus, you find it necessary to draw it a little closer in proportion as the tumor has already yielded. Not unfrequently it happens, that at this time the probe or catheter comes easily away; but if the ligature continue to retain its hold, it is but for a day, or at the utmost two days longer; and though the pain is not renewed, the polypus being now less sensible, the parts are still more blackened, the discharge is extremely fœtid, thin, and copious; some blood usually flows at this time, the swallowing improves, and the hearing grows too acute, irregular, and confused; the tube falls away on the sixth, seventh, or eighth day, and often it happens, that the tumor melts away so entirely, and is resolved into this gangrenous ichor, that no perceptible portion of it falls away: sometimes, continuing more entire, it drops into the throat and the patient rejects it; often when it drops into the œsophagus it is swallowed, and is passed ingested by stool; sometimes the patient is conscious of having lowed the tumor, but more usually it passes over the throat insen and during sleep. I have known it happen, especially in the han ignorant people, that after the polypus has dropped off, and act been passed by stool, the tube and wire have kept their place, without the reason being at all suspected: it is this, that the loop of the ligature is larger than that narrow slit of the nostril through which it should pass, and thus it hangs suspended, and I have known it hang so a month, but loose, moveable, and easily taken away. I do not know that the loop of wire ever needs be left, even in the most bulky and cartilaginous polypus, beyond the eighth day."

An interesting case is narrated of two large knuckle-like heads, or tubercles of a polypus of the posterior right nostril, projecting below the soft palate, and so protruding, as to have led a country surgeon to mistake them for suppurating tonsils, which he accordingly scarified, dividing the velum and polypus to the extent of an inch on either side. In this case the above described operation succeeded perfectly in the more skilful hands of Mr. Bell.

In speaking of the third or last stage, our author presents a most pathetic picture of the sufferings of a fine young man who lingered for the space of a twelvemonth a living reproach to surgery, and to the latest horrors of whose most miserable existence Mr. Bell manfully and generously attempted to put a period; but was overruled by nature. The *finale* of this discourse is consentaneous with that of "a good woman," who fell a victim to inflammation of the brain from a rude effort at extraction in the advanced stage of her malady.

Discourse VI. *On tumors of the gums, lips, cheek, and throat.* The following is a description of the appearance and progress of a fatal tumor of the gums.

"The gums are subject to such an infinite variety of slighter swellings, to boils, to little indolent tumors resembling the hordeoli, or tumors of the eye-lid, and to harmless indurations, that those which are the germs of the most horrible diseases, are too little suspicious, too slightly characterised by any peculiarity of form or aspect to attract attention, till too late. A small, firm, seed-like tumor is perceived, but hardly noticed, for months; it is seated between two of the fore teeth, begins to separate, displace and loosen them; it grows imperceptibly and slowly: the teeth are raised from their sockets; they become loose and vaccillating; they have no longer any hold of the jaw, but are merely hanging in the tumor; the tumor retains its original gristly hardness at its basis, becomes rugged and irregular in its upper part, with a cock's-comb-like edge: though little painful, it bleeds from time to time; it extends itself to the gums of other teeth, which are successively displaced from their sockets and loosened, and one side of the mouth is occupied with it. Next the hard basis of the tumor extends into the cheek; the tongue begins to be pushed aside, horrible fœtor of the mouth ensues, with frequent hemorrhages; the tumor now protrudes and keeps the mouth open; the disease becomes now terrible and painful, the patient survives for a few weeks, with the mouth and lower part of the face wrapped up in handkerchiefs, in a miserable and loathsome condition; the putrid and blackened blood distilling with the acrid saliva through the filthy clothes, till wasted by suffering he dies in inexpressible pain. Such is the kind of death, from which, I sincerely believe I have saved many by a slight incision."

This description is followed by a case, which admitted of a successful operation; and as far as it proceeds, so closely illustrates its several points, that we cannot but suspect the former to have been chiefly founded upon the latter. We have known a case in which wine and lemon-juice dissipated precisely such a tumor as that next described, respecting which serious suspicions were entertained; but we forbear from making any application of this fact. The following is a pleasant prognosis, styled an "opinion,"* on the case of a gentleman who had a fungous tumor attached to the lower jaw and filling the mouth.

"Mr. Keil cannot have witnessed the doubts and difficulties of his attending surgeons, without being aware of that imminent danger, which it falls to my lot to announce to him; *and my duty and conscience equally incline me, to declare it without loss of time, and without reserve.* A tumor so unusually rapid in its growth, cannot cease to grow, but must continue to extend the disease by increasing the caries of the jaw-bone, and propagating the morbid action among the surrounding parts: the hardened basis of the fungus will soon spread into the cheek; abscess, and indurations of the lymphatic glands will ensue, each ulcer will throw out its fungous efflorescence, and the disease will burst out upon the face and angle of the jaw, *with all the virulence, and more than the deformity of a real cancer;* this resembles in its form that fatal fungus which sometimss sprouts from the axilla, after amputation of the cancerous breast, or that which still more frequently proceeds from the spermatic cord, after extirpation of the schirrous testicle, and which no methods, *even the most severe and rude in surgery, have ever been known to subdue.*"

An operation, being advised, was submitted to; but the event too truly confirmed the "opinion."

From the verge of the mouth we descend rather abruptly to the verge of the anus, but the *bathos* is ingeniously broken by an intermediate reflection upon the singular contrast of the malignity and sudden growth of tumors in the former with the indolence of these in the latter situation; and a case clears up the mystery of our fall. We are referred to the sketch of a tumor four inches long, which depending like a polypus from the wall of the rectum, lies coiled within the gut, and is bolted out at each time of going to stool; but we have searched in vain to find it. The case book gives another diversion to the subject; it leads us to the consideration of those watery yet bulky tubercles which surround the verge of the anus, and are infinitely more frequent than those within the gut. An ungainly attempt to kill them by ligature inclines them to cancer. In such a case, where the basis of the tumor was very hard, and the tumors towered to a great height, where *all the verge of the anus* down to the very circle from whence they arose, was *soft, limber, and natural,* as if no such disease were even in its vicinity, our author *spared nothing;* he cut out with each tumor *all that part of the verge of the anus that belonged to it,* muscular as well as cutaneous; to *whatever depth the hardness extended* he cut, introducing the bistoury quite *within the circle of the anus.* I cut away, (he repeats with a chivalrous exultation) the whole circle of the anus, and have so often cut away the whole, or much of the circle without the slightest ill consequence, &c. The most noisy declaimer against cruelty of whom we know any thing, is Mr. Bell. The word very absurdly recurs whenever an allusion is made to obsolete surgery, as if the imputation of cruelty could attach to any man who acted from humane motives. We cannot retort his own reproaches upon himself, for he professes that his object was to rescue the patient from a miserable death. But what satisfaction we should be glad to know does the patient receive for unnecessary torture, from being assured that his surgeon's motives were good. The monstrous incongruities and contradictions of this account compel us to pronounce, either that Mr. Bell did not exactly know what parts he cut, or that he cut more than was needful; and the question, *uter horum?* will best be decided by himself. Glandular tumors seated in the centre of the cheek, firm, hard, and knobulated, being interspersed with globular masses elastic to the touch, and containing a small portion of gelatinous matter, come next to be considered. They are covered with a coarse, granulated, porous skin like the rind of lemon, adhering to the substance of the tumor. This is accurately exemplified in the case of an Irish gentleman, related both by himself and his surgeon, with all the particulars thereunto pertaining, and occupying fourteen pages. For the reader's further edification he is presented with four several sketches of Mr. Taylor and his cheek. In the course of this narration, which is upon the whole more creditable to our author's candor than judgment, but most of all to his operative skill, we meet with a note expressing an opinion which we have long entertained on the practice of dividing nerves in the operation for the tic doleureux.

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* May heaven receive us and our friends into its eternal felicities before death comes in the *unquestionable shape* of an "opinion!" The unhappy patients may remonstrate like the frogs in the fable; "Gentlemen! this may be sport to you, but it is death to us."

"I doubt not the reason of the tic douleureux returning, is often like the ill success of vaccine inoculation, to be ascribed to the manner in which the operation is performed. The performing of this nice and delicate stroke of the knife was probably confided, in this case, to a Gentleman whose first and only dissection of nerves had been in the *living subject;* his ill success was sufficient warrant for this conclusion; it was impossible for an anatomist to look at the scar of his incision, without an instant conviction, that it did not at all cross the course of the infra-orbitary nerve; deceived by the baggy eye of a man advanced in years, and feeling only for the margin of the socket, he had carried a long incision in the direction of the natural lurk of the skin, and certainly could touch only the rim of the socket; it is far below this, that the nerve lies in a deep hollow: my education, and my experience, put me, I believe, far above the puerile vanity, and I hope above the suspicion of magnifying beyond the truth, this trivial success; I could not but know the infra-orbitrary nerve a little better than my predecessor, and could not fail to cut it. I seated my patient in a chair, and pressing the point of my fore-finger deep into the hollow where the nerve lies, cut it across, by striking in a small sharp-pointed knife, making no length of outward incision, and, hooking the point of the crooked bistoury behind the nerve; in the very instant of the stroke by which it was divided, Capt. G— started from his seat, ran forwards in great confusion, exclaiming, "Good God! what's that?" he sat down instantly in perfect composure, free from pain, unconscious of the operation being performed, and expecting it: when questioned about the sensation which made him start up, he said he felt nothing but as if he had been shot in the head, followed by a momentary confusion, and a total relief from pain. He said he did not imagine the operation done, because the first operation had been a deliberate dissection; he felt now perfectly confident that he was cured, and returned home in two days, since which time he has lived in perfect health, is happily married, and continues well at this moment.

"I have only to observe, that those gentlemen who have the happiness to be born operators, and to be endued with such natural genius and talents as to be emancipated from the wearisome and disgusting tasks of study and dissection, though they are of course very successful in amputating limbs, cannot expect to be equally so when the question is about dividing a nerve."

Seven succeeding pages are occupied with an account of the removal of a cancerous parotid, the subject of which, as might be expected, died of a return of the disease. Numerous memoirs of "hairbreadth 'scapes and moving accidents" conclude the discourse. We read of deep-seated tumors behind the angle of the jaw,—adhering to the pterygoid process and back of the pharynx,—occupying the whole side of the neck, and pressing upon the wind-pipe,—covering at once the neck, face, and jaws, —filling up the angle betwixt the ear, jawbone, and neck,—and lastly, braced tightly down by the mastoid muscle upon the sheath of the carotid artery.

It may seem invidious, but it is nevertheless an imperious duty to point out any gross incoherence of a writer's principles and conduct. In the case of the afflicted Jenny Brown, the author says, "I imagine there are *many safe and easy* ways by which the tumor, great as it is, might be extirpated or destroyed." Now we will ask Mr. Bell one simple question. Why did he not, instead of pencilling and prosing about it, extirpate or destroy it? Where was his favorite maxim,—"*Remediis, non eloquentiâ morbos curari!*" Because forsooth a consultation has decided otherwise. Delicate benevolence! Fastidious humanity! Turn, reader, to another in this volume of cases. (p. 246.) "I felt now that *professional ceremonies* should give way to *essential charities.*" (and again p. 248.) "But to decide *thus* is to forfeit the natural advantages of *our own deliberate judgment.*" With what shew of justice then are the more diffident of our brethren incessantly rated for wishing "to wait the event of time," to "make a trial to relieve," "to proceed as circumstances will admit," &c. Have they enjoyed opportunities so far superior to Mr. Bell's, as to be without apology where he is without fault; to be abused for indecision, where he is at liberty to palter? Away with this declamatory babbling of others' ignorance, and others' failures. That indecision is indeed culpable which accompanies superiority of knowledge, and those errors conspicuously glaring which are committed and deprecated by the same hand.

Discourse VII. *Of salivary tumours.* The following is our concluding bill of fare. A case of enlarged submaxillary gland accompanied by observations, notes of the operation, and some simple rules for operators, where large blood vessels are concerned. A species of vascular transparent tumor which appears upon the tongues of children, subsides on puncture, but requires to be cut freely out with scissars. Sacculated salivary tumors containing pellucid gelatinous matter very indistinctly felt, owing to the thick fleshy masses by which they are exveloped, and growing to an alarming size; having a solid stool or nucleus which cannot be discussed, and which proves fatal if not extirpated. A tumor of the sublingual gland filled with a mixed matter resembling mustard, having a tense and almost solid feel from the compression of the lingual muscles. A fluid sacculated tumor of a salivary gland, extending from the lobe of the ear to the sternum, displacing the mastoid muscle, and after much mal-treatment, cured by strong injections of port wine. An aneurism of the carotid artery, situated under and behind the angle of the jaw. A further description, with cases, of the ANEURISM FROM ANASTOMOSIS; varicous swellings about the thyroid gland. An extraordinary instance of bloody effusion into the anterior chamber of the eye, from rupture of the vessels of the iris, preceded by head-ach and obstructing vision; a bloody tumor encircling the throat, cured by a seton drawn across the sac. Lastly, histories of deep scrofulous abscesses allowed to burrow among the soft parts, compressing the throat, closing the arches of the palate, and bursting into the pharynx.

We are informed (p. 250.) that, "aneurism of the carotid artery *is indeed so rare a disease that it is not certainly known whether such a disease exists*." "Nothing could more tempt us to a *daring experiment* than the desperate condition of such a patient, nor is there any thing in the relative situation of these parts to deter us. Had this woman been under *my* care, or should even such a case recur, I should never hesitate one moment, conscious that the most absolute bungler in surgery might lay aside the muscles with a few strokes of his scalpel, the common sheath of the carotid, and its accompanying nerve, and separate the vein, nerve, and artery, so as to tie the latter without let or hindrance." Aneurism is a rare disease in Scotland, and a carotid aneurism never having until now occurred within the experience of Mr. Bell, therefore carotid aneurism is so rare a disease that it is barely known that such a disease exists. There are no mountains in Middlesex; the topographer of Middlesex never saw a mountain: *ergo*, there are none in England. We will not search for recondite authorities, but content ourselves with referring Mr. Bell to "the medical intelligence" of our last number (if it has been so fortunate as to find a place in his book-room) in proof of the occasional existence and cure of this disease. We will likewise take the liberty to inform him, that, in addition to three successful operations for carotid aneurism, many have been seen in London, in which no operation was practicable; and that we are at this moment acquainted with a case shortly to become the subject of operation. But if this *daring experiment* was level to the capacity of an *absolute bungler*, how fares our friend the author? Why wait for the recurrence of such another case to prove that nothing more was required, than with a few strokes of the scalpel to lay this part aside, and to open that, *without let or hindrance?* Even eloquence here comes too late. In the language of proverb, "the stable door is shut when the steed is stolen." This, as well as the case of poor Jenny Brown, are practical estimates of the difference between talking and acting. If Mr. Bell always displayed in the latter, the promptitude and dexterity of the former, he would not cede the palm to any man living as an operator in surgery.

To the existence of aneurism from anastomosis, the *protegé*, if not the legitimate issue of our author he attributes many of the most mortal diseases of the internal viscera. We shall extract the distinction given between this disease and varix.

"But the disease, which I have in a former volume described under the designation of 'ANEURISM BY ANASTOMOSIS,' proceeds from something very different from a mere dilatation of veins: the disease is not formed till an essential change of structure, and a new mode of circulation has taken place. Varix is a simple dilatation of veins produced by time and long use, by stricture upon the great returning trunks, by swelled glands, by imprudent ligatures, by pregnancy, by much walking as in soldiers, labourers, or running footmen; it is a dilatation unsightly at first, benumbing and painful next, and highly inconvenient and oppressive in the next degree: annihilating gradually the secreting parts of the glands, as in the testicle, liver, &c. or ending in destruction of parts, in ulceration with hemorrhagy, when seated in the limbs. But aneurism from anastomosis is an entire change of structure, it is a dilatation of veins, in which they do not give way from natural debility, are not distended by any kind of constriction, but are forced and enlarged by the diseased action of their corresponding arteries. These happen first from a blow, a pinch, or a laceration; or, in consequence of original mal-conformation, a violent action of the arteries, and a mutual enlargement of arteries and veins; while the intermediate substance of the part is, by this impulse, and, in course of time, slowly distended into large intermediate cells, which are dilated at last into formidable reservoirs of blood. The altered structure of the part resembles then that imaginary parenchyma, or cellular substance, which the early anatomists of Europe presumed, (but indeed they pretended to prove this by injection,) was interposed betwixt the extremities of the arteries and those of the veins in all parts of the body, especially in the secreting viscera. They imagined they could make such intermediate cells visible. This they entirely believed, and as yet we know not how far to condemn their theory; but in the disease I now speak of, it is a fact, and we describe nothing but what we can almost see. The blood is poured into the cells of such a tumor by innumerable arteries, whence it has even in the beginning an obscure throbbing, and in the latter stages a strong and deep pulsation: from these the blood is continually flowing into veins, which, in place of imbibing it by absorption, receive it with such patent orifices, that in a few moments, and by a very gentle pressure, the largest tumor of this nature may be compressed, and all its blood repressed into its veins, leaving only a doughy feeling of thickness. The veins form a conspicuous part of such a tumor, but the intermediate cells are as sensible a part of the structure; for when the tumour is thus emptied, we feel that the blood is repressed from the sacs in the veins; and, when the tumor is large with a purpled surface, we feel the sacs individually prominent; when they burst, or are punctured, we see the blood well out from them; and when the tumor is extirpated, they seem to form its chief bulk: every pulse of the heart, every action of the arterial system and of the affected vessels, is accessary to the increase of the tumor, which as often begins from a pinch, a blow, or speck of ulceration, as from native mal-conformation; and which, though it begins in a spot, is destined to increase to an unlimited size, at least, till the parts can no longer bear the force of the circulation, but become thin and burst.

"The character of this tumor results from its structure: it is a tumor beginning often from a slight injury, or imperceptible cause; increasing from a slight thickening to a tumor of some magnitude, but still colourless, void of pain, soft, inelastic, of a doughy consistence, and having a woolly or cushion-like feeling, when pressed and moulded under the fingers: it is greatly inflated by violent exertion; especially running, straining, struggling, or repressing the breath; and it recedes slowly upon pressure. But it is colourless and void of pulsation only in its early stages; when the cells are enlarged into sacs, and the mutual communications consequently free betwixt the extreme arteries and veins, the whole tumor pulsates distinctly, and, when excited by exertion or muscular struggles, it throbs furiously; the tumor assumes then a purple hue, the apices of the sacculi, become sensibly thin; the patient is alarmed from time to time with slighter hemorrhages, which becoming more frequent from various points, and very profuse, he is at last debilitated, changes his complexion and colour, loses his health, and dies."

The case and portrait of a young gentleman of Birmingham, in whom this formidable disease was seated between the eyes, stretching upwards on the forehead and downwards on the nose, and who ultimately expired of hemorrhage, are introduced by a letter of consultation from Mr. George Freer to Mr. John Bell. To this succeed copious observations on the disease and instructions for the operation, in the shape of a "reply." A second letter comes from Mr. Freer in answer to Mr. Bell's request to publish the case, in which, among other things of equal importance Mr. and Mrs. Freer promise themselves much pleasure in paying every attention to Mr. and Mrs. Bell, and shewing them the button manufactories. This is one of those luxuriant and spreading *fungi*, for which, however cruel it may seem, we recommend nothing short of actual extirpation. Our author speaks somewhere vaguely of blood being matured into pus, &c. but of this knotty point, as well as that of periosteum secreting bone, we shall defer the consideration at present. The sad variety of wretchedness marking the case of which the following is the sequel, would have beggared any other than this writer's description.

"It was in this condition, in this last stage of his malady, that I saw this wretched man: he was meagre, and *staring with famine, his belly clang to his back*, his skin to his bones, his face was squalid, lean, and yellow, his voice hollow and rattling, his grey eyes sunk in their sockets, the eyelids and skin of the forehead raised and wrinkled in deep furrows: whenever he moved, he was in danger of suffocation; the slightest attempt to swallow was accompanied with a struggle which he seemed hardly capable of surviving: his *skinny hands*, and *sprawling lean fingers* were perpetually extended before him, as if ready to catch at something for support, when the strangulation came upon him: he had neither strength nor voice, to tell me the date nor the cause of his sufferings, and, though the abscesses on each side of his jaws extending along the neck, varying in their state of fulness, and bulging upon every attempt to swallow, explained his condition in part; yet in honest truth I mistook, at first, the slit-like opening for the way into the œsophagus, till I found that, in place of admitting the lithotomy staff, which I used as a probe, to pass downwards into the pharynx, it only entered when turned with the point upwards towards the nostril. Satisfied and instructed in this point, and knowing part of the disorder, viz. that the arches of the palate had closed and adhered, I sought the passage downwards, and having passed the flexible catheter into the œsophagus, I poured down by the syringe a bason of warm beef-tea, the first meal he had enjoyed for a long while, and which he acknowledged to be very grateful. But neither this first operation, nor any subsequent one, was easy; I knew nothing precisely concerning the ulcerated openings leading into the sac; often when the catheter seemed to pass into the œsophagus, it *plumped* into the sac, its point was resisted, and it was only by its stopping short thus, that I was aware of the tube having missed the pharynx; upon such occasions, I withdrew it to try again for the true passage: the catheter was always passed two or three times, before it actually entered the œsophagus; it was only when it passed easily and quite down into the throat, that I durst venture to pour in the soup; sometimes it went into the opening of the larynx, and, though instantly withdrawn, excited such a suffocation as was very alarming. I fear that the pupil to whom I entrusted the passing of the catheter, and the nourishment of the patient, had actually failed, and concealed his own awkwardness from me; I cannot but reflect with self-reproach on my own remissness in not passing the catheter regularly myself; but, I believe, this complicated disorder was quite incurable; the poor man grew more ghastly in the visage, and weaker every hour; and, while we were engaged in contriving how the sac might be opened, or what should be done, he expired on the fourth day."

The proposal to inject the most solid tumors, so as to restore by moderate violence the cellular interstices obliterated by disease; or to inject the surrounding *tela cellulosa* of tumors which refuse to suppurate, with oily and camphorated solutions, is at least original.

Appended to the volume is a "*Memoir on the Surgical Diseases of the Poor,*" at least addressed to the surgeons, clergy, and heritors in the remote parts of the country. This is a very curious composition. It is pretty generally known that Mr. Bell has ceased for some years past to hold any official situation in the Royal Infirmary of Edinburgh. The design of this memoir is, in our opinion, highly reprehensible. It is to enforce the objections, already too widely disseminated, which the poor raise against public charities, and to confirm their prejudices by affecting to compassionate and supercede them. But is it the part either of liberal science or enlightened philanthropy, to talk of the *disgrace* of going into an hospital, of the *shame*, the *unhappiness*, the *pains* of a public operation, the *agony* of being exposed before numbers of spectators, and above all, to call such feelings *just*? Contagion, noise, confinement, bad air, the cries of those under operation, the sight "of his fellow sufferer conveyed to the scene of trial, or carried back in silence and solemnity to his bed, to wait the issue, life or death;" his dying groans, his nurse's tales are worked up in explanation of the poor man's antipathies. Add to these the report, "that an infirmary is not a simple charity, not founded solely for him, and such as him; that it is reputed as a school of experiments, and not much famed for successful operations;" the exposure "of his sores and his sufferings to a hundred spectators," and the belief, "that even his remains would not be respected should he die." These are circumstances, says our author, in which the efforts of one individual, of acknowledged skill, the pledged and proffered services of one in whose humanity and talents the lower people may confide, may be a blessing to the poor, over all the country. All this is of course introductory to a proposal to give advice, and perform operations gratuitously; and to publish "an annual volume of consultations," We have nothing to do with the medical politics and parties of Edinburgh; nor have we any power, and much less any inclination, to arraign the motives of any public man. Nor do we call in question the justness of the author's self-appropriation of acknowledged talents, skill, and humanity. Further, we admire the spirit which actuates a professionalist of eminence to devote much of his time and pains to the diseases of the poor: it argues, abstractedly speaking, much zeal and much benevolence. But we know not how to admit the disinterestedness of an insidious attempt to curry favour with the poor, by affecting sympathy, not with his real grievances, but his absurd and silly prejudices; and instead of studying to compensate for the narrowness of his education by inspiring him with generosity and confidence, labouring to foster and keep alive the chimerical suspicions and ungenerous fears, which were inherited from his grandam, and will be instilled into his child. So diametrically opposite are our feelings and opinions to those of Mr. Bell, that we hail with the highest satisfaction every opportunity of reasoning the poor man out of prejudices painful to himself, inauspicious to science, and injurious to society. For this end, all must be candour and openness on our part, and an example of the conduct which we wish him to pursue. He must repose confidence in our disposition, as much as in our skill; and every circumstance mars the effort to ensure this confidence, which does not operate to diminish the distance between our mutual lots and fortunes. For our parts, we should heartily rejoice to see the day when no patient objected to enter a hospital upon the express condition, that in the event of death, his attendants should exercise an official prerogative to inquire into its cause. We are convinced that very many of the poor of this city are ripe for such an edict. The policy of its utterance is another and more extensive question. We hope and believe, that the sarcasm of Mr. Bell upon the *treatment* usually received in hospitals, is as gross a libel as that which he has chosen to pass upon the *motives* of the medical officers. No man can act upon the detestable principle—"*Licet experimentum in corpore vili*"— who admits as binding the first and fundamental law of practical morality. But where (as he admits) diseases are most formidable from the progress they have been suffered to attain, remedies must necessarily be administered with a boldness and decision suited to the urgency of the occasion; and whether they prove sanative or prove injurious, leads to no other than this conclusion: that the art itself is still so much of it experimental, as to justify doubtful practice in doubtful cases, and establish beyond all controversy the wisdom and benevolence of the founders of public hospitals. But with what grace does the author of this book of cases vent his splenetic reproaches upon those very sources of observation and schools of experience, from which he has directly or indirectly derived knowledge, reputation, and subsistence. To set the matter in a clear, though very limited view; had *he* not been nursed in a hospital, what copious stores of instruction would have been for ever lost to the student. Even the volume which is so concluded, was so composed. If there is in Europe a hospital surgeon who entertains such notions as Mr. Bell has unhesitatingly expressed, he is a viper in the bosom of his benefactor, "which doth mock the meat it feeds on."

We are conscious that this article has already exceeded its proper limits. A few summary observations, on the composition and style of the work, shall conclude it. General descriptions assuming the air of "doctrines," are seized upon by the young student as fixed points for his observation and guidance, and they are in fact the only parts of a work like this, that can be carried away and converted to his practical use. They should then be most cautiously drawn. But in reading Mr. Bell's we much fear, that from a premature aim at comprehensiveness, his laws are propounded, and his histories built, upon narrow observations, and insulated facts. Whoever will compare the general description of aneurism* with the details of a certain remarkable case recorded in the Edinburgh Physical and Literary Essays, and moreover observe throughout the productions of the same pen, the apposite allotment of a solitary illustration to a sweeping dogma, will be convinced that there is some foundation for our fears. In our belief, there does not exist a more injurious faculty of mind. Quickness of apprehension, a warm imagination, and a situation so conspicuous as to demand somewhat of oracular promptitude in decision, are the chief circumstances which combine to produce it. To these prematurities of judgment, a man of quick conception and lively genius, though unquestionably most prone, is unfortunately by no means most sensible. For example, what man of delicate feeling would not be covered with confusion, upon reading the irrevocable memorials of his own culpable haste and self-sufficiency? but more than ordinary practice does not teach a man extraordinary care, and he who writes *often*, soon loses that distant respect for his readers, which is the best security for his writing *well*.

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* Principles of Surg. Vol. I.

Mr. Bell has been too long and two extensively known to the medical public, to make it necessary for us to adduce the specimens with which these pages are literally loaded, of offensive egotism, overstrained description, and colloquial vulgarity. He is always rhodomontading with his pen, and always caricaturing with his pencil, and the design that pervades the whole, seems at least as much, to make us merry as to make us wise. The engravings are the best executed part of the work. Of the plates, so many are altogether omitted, and so many defective in reference and confused in arrangement, that it is seldom possible to connect them with the description.

Let it not be objected to our remarks that they have savoured of personality—we can affirm that we are never actuated by so despicable a motive. But there is a description of writers who so invariably present *themselves* with their opinions, between whose thoughts and *characters* the cement is so strong, that the critic, however he may desire it, struggles in vain to look at the matter and not see the man. We should have spared our time and pains about Mr. Bell and his writings, if we did not regard the last as so many public and lamentable prostitutions of the rare and excellent talent with which he is endowed. We strenuously recommend the "*nonum annum*" to his attention, and seriously regret that he has not observed it heretofore; for, if he had written much more, he *must* have written as well, and if he had written much less, he *might* have written better.

Art. V. *A Treatise on Inflammation and other Diseases of the Liver, commonly called bilious; including the Synochus Biliosa; Cholera Morbus; Torpor; Schirrous; Biliary Calculi; which is preceded by a short Description of the Structure of the Liver, and the different Physiological Opinions respecting the use of the Bile. Lastly is added a Monthly List of Diseases, from the* 1st *of June* 1806, *to the* 30*th of June* 1807, *with the State of the Weather and Thermometer.* By W. White, Member of the Royal College of Surgeons, London; Surgeon, &c. to the Bath City Dispensary, and Asylum. 8vo. pp. 149. 5s. Bath, Hazard and Binns; London, Cadell and Davies. 1808.

WE notice this little volume rather from the importance of the subjects on which it treats, than from the value of the opinions which it contains. The title page reads well and promising in the list of new publications, and might lead persons, unless cautioned by their reviewers, into expense, trouble, and disappointment. Indeed when we took up the book and read over the long list of subjects which it professes to discuss, and ailments which it professes to describe, we did for a moment believe, what our experience of the present state of literature ought to have prevented us from believing, that no man was absurd enough to sit down to the composition of such a work, without some excuse for publication, either in the novelty of his reasonings, the value of his experience, or at least in what we are not often willing to receive as an apology for authorship, in his extensive and erudite reading. A perusal of the preface however considerably altered our expectations, and convinced us that Mr. White and ourselves hold very different opinions about the nature or the degree of those motives, which ought to persuade a man to become an author. In the preface he writes:—

> "In no specific Treatise on Diseases of the Liver, that I have met with, is the synochus biliosa, particularly noticed. This circumstance alone is in my opinion a sufficient apology for the present undertaking; for certainly this disorder forms an important link in the chain of hepatic diseases."

Now it requires no very great powers of argument to shew that our want of a description of the synochus biliosa is an apology for describing it, and for nothing more; that the above is an excuse for a trifling part not for the whole of the volume; and that although it might be very proper for Mr. White to describe a disease minutely which before had been described imperfectly, yet there was no necessity for loading it with such a multitude of other matter, old, familiar, or trifling. If Mr. White should send his coat to a taylor with orders to put on an additional button, and he were to receive not only a new button, but an entirely new coat, we conjecture that Mr. White would think his taylor a very forward and impertinent fellow; and yet as the taylor would have behaved to Mr. W., Mr. W. has behaved to the public. Because something remains to be said of liver diseases he takes occasion to recapitulate what had been said long before, and behaves himself just like a school boy who is unable to tell how much is twice nine without first beginning with twice one and twice two.

We shall examine this book, chapter by chapter, and report progress as long as we have patience to do so. We do not expect to find much information, yet the task may have its usefulness. It may teach us, that a man will sometimes publish without one reasonable motive for publication; it may shew us that books will sometimes be filled with chapters which contain little more than a statement that this man held this opinion, and t'other man the other; and may prove that the mind as well as the body is liable to effusions which depend on other causes besides plethoric fullness.

Mr. White begins by stating that before he proceeds to consider the diseases, he must describe the anatomy and physiology of the liver. Why this prefatory matter is necessary he leaves his readers to guess. With a laudable hatred of all unnecessary trouble he resolves never to make anew what he can find ready made by others, and dispatches the description of the healthy anatomy of the organ by a modest extract of six pages from the well known essay of Dr. Saunders: our author adds this novel and sagacious inference "that the principal design of the liver is for the purpose of biliary secretion." He then slightly hints that physiologists have differed about the part of the liver in which the office of secretion is performed; tells us that the hepatic duct joins the cystic duct, that the cystic duct comes from the gall bladder, and that the gall bladder lies in a depression on the concave surface of the liver, and then filches another extract about the analysis of the bile from his useful friend Dr. Saunders.

Next follows a disquisition on the uses of the bile. This disquisition is made up by, 1. An extract from a paper of Dr. Rush, in the Med. & Phys. Journal, in which he states it as his belief that the bile is totally absorbed by the lacteals and converted into nourishment. 2. An extract from a treatise of Dr. Coe, to prove that Dr. Coe and Dr. Rush believed alike. 3. An extract from Dr. Saunders to prove that he believed differently to both. 4. An extract of seven pages from the essay of a Mr. Saumarez, in the Medical and Physical Journal. This gentleman takes up the cudgel against Dr. Rush, and if we were to judge from the plaudits with which our author greets every syllogism from his pen, uses it with effect; although if we may judge from his trifling and his obscurity, he is as driftless and muddle-pated an arguer as ever wielded a pen. The physiological doctrines of this writer quoted by Mr. White with so much applause, are mostly unsound; but they are either too unimportant to deserve refutation, or are safely guarded from any assault by the tough shield of incomprehensibility. With ten more extracts, one from Dr. Coe, one from Dr. Cullen, two from Galen, and six from Mr. Pearson relating to the definitions of inflammation and proximate cause, concludes the chapter on the anatomy and physiology of the liver. It is a useless piece of patch work made up of common patterns, to which our author has contributed nothing but the rotten thread with which it is sewed.

Hepatitis. Mr. White thinks that the symptoms which distinguish inflammation of the concave from that of the convex surface of the liver are very equivocal, and gives the case of a woman who was attacked with symptoms of inflammation of the concave, but suffered suppuration of the convex surface. He also thinks that when the membrane of the liver only is affected, it is by an *erisipelatous* inflammation. This chapter concludes with a detailed statement of the common information concerning the symptoms, the causes, and the treatment of hepatitis. It contains nothing but what is perfectly familiar to every medical practitioner of moderate information.

The chapter on chronic inflammation is even more empty and more trifling than the former. We would tell our readers what it contains, but really it is not worth reporting.

It may perhaps be thought that we have been unnecessarily and unjustly severe on those parts of the volume which we have hitherto been employed in examining. But although we may possibly be accused of harshness we at least lay claim to sincerity, for in the foregoing observations we have only written down the thoughts and feelings which we experienced on perusing the dissertation of Mr. White. In the capacity of reviewers such a load of literary trash passes under our observation, that we become tired of the useless and disgusting labour of reading what cannot boast of one decent claim to attention, and thus acquire an unusual irritability of temper towards this class of grievances. As we are anxious to convince Mr. White that we have no desire to treat him with unjust severity, we shall extract that part of his book which he himself seems to value the most highly, the description of the synochus biliosa.

"There was nothing however uncommon in the symptoms of this disease when it first appeared; the leading characters of it were chilliness, sickness, vomiting, and diarrhœa; generally attended with pain in the bowels, back and head; sometimes with cramp in the lower extremities. The tongue was commonly covered with a white and yellowish mucus. In general there was an increase of heat of the skin, and quickness of the pulse.

"In the spring of 1798, the disease assumed a more complicated form, putting on, in several instances, the appearance of acute rheumatism; the pain was mostly about the face, ears and neck. In one case, where there was a violent pain and swelling of the right side of the face, it was followed by a paralysis of the muscles of that side: In two other cases, there was a paralysis of the eye, which succeeded to the violent pain*. It is necessary to observe, that the connexion of the above symptoms was clearly evinced by these pains alternating with those of the stomach and bowels, as well as by a redundancy, and vitiated state of the bile, occurring at the same time. As the spring advanced, the disease put on more of a pleuritic form; and that again in proportion as the heat of the weather increased, was combined with cholera. Sometimes it happened, that a person was attacked with only one of these varieties of the disease; but very often the same person had a combination of the cholera with the pleuritic, or of the choleric with the rheumatic.

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* "This combination of rheumatism with the bilious fever was often accompanied and followed by paralysis of the limbs, which however, generally yielded in a short time to the usual remedies."—Dr. Pearson.

"Since the summer of 1798, I think, all the varieties of the disease have been more indiscriminately intercurrent with each other.

"Very often the disorder assumed the appearance of dyspepsia and hypochondriasis, and those who are not sufficiently acquainted with its nature, and different modifications, will no doubt, be very liable to mistake these symptoms for original diseases; and indeed, unless the complaint be very extensively seen, it will be extremely difficult to detect it in its various forms. I have known a variety of nervous medicines given for a length of time in such cases, without any benefit being derived from them; and yet after exhibiting a pretty brisk purgative, as calomel and jalap, patients have been speedily relieved from the most distressing feelings. Sometimes an emetic has been given in those cases with obvious advantage. In the last mentioned forms of the disorder, the bowels are generally very torpid, the pulse is often quick, and the tongue white; but perhaps the latter symptoms are not so particularly attended to by the practitioner, as they ought to be; the patient's description tending very much to impress his mind with the idea of the disease being either dyspepsia or hypochondriasis.

"Having given a general view of the disease, I shall now proceed to give a more circumstantial detail of the diagnostics peculiar to it.

"For the most part it commences with a violent shivering fit, which afterwards recurs, but not so severely as at first. Sometimes it happens, that there is no chilliness felt by the patient till three or four days after the commencement of the usual febrile symptoms. At other times, the disease is ushered in by a sudden giddiness, and dimness of sight; and under these circumstances the person often falls down, and remains insensible for some time. Frequently, also, pain in the side, and bitter taste in the mouth, are the primary symptoms; and not unusually a numbness either of the upper or lower extremities.

"The pains attendant on this complaint are very general, and very shifting, being felt in every part of the body in the course of a few minutes; though in general they are more permanent in the stomach and bowels than elsewhere; also betwixt the shoulders, and in the sides.* Very often there is a pain of the back, as low as the os sacrum; and from its manner of attack, very much resembling violent paroxysms of spasm[*].

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* Although the pain in the side, so frequently occurring in this disease, be very violent, occasioning a considerable interruption in respiration, yet it is not so fixed as in true pleuritis, often alternating with pain in some other part; and is not attended with so much cough.

"The head is commonly, though not always, affected with pain, and particularly about the eye-balls; not always fixed, as above hinted, but alternating in general with pain in some other part.

"There is a great disposition to drowsiness, but seldom much suffusion of the eyes, or confusion of ideas.

"Pain, swelling, and inflammation of the large joints, resembling acute rheumatism, are frequent attendants; with this difference, that the inflammation occurring in this disease sometimes terminates in suppuration and ulceration. In a case of this kind which happened, there was also a caries of the bones of one of the toes; and it is very remarkable, that as soon as the morbid action of the toe ceased, a morbid state of the bowels (which had been previously affected) immediately recommenced.

"The pulse in general is pretty strong, full, and frequent; but sometimes neither its strength or frequency is increased, although the other symptoms indicate the presence of fever[†].

"This circumstance happens indiscriminately, to those patients who are able to walk about, and to those who are confined to their beds:—the pulse does not vary much in the course of the disorder, even when it continues for several weeks; in some few cases, however, which proved fatal, the pulse towards the termination, became small and weak.

"The state of the skin is variable; sometimes its heat does not exceed the natural temperature; but in general there is an increase of heat, often so intense as to communicate an unpleasant tingling sensation to the fingers, on feeling the pulse. Frequently the patient is relieved by the breaking out of a sweat; at other times no abatement of pain ensues, although the perpiration continues the greatest part of the night.

"Jaundice is another symptom which occurs in this disorder, and *which is not occasioned* in general by any obstruction in the biliary ducts, but from a redundant secretion of bile, as considerable quantities have been discharged from the stomach and bowels. It has happened, however, in a few cases, that the passage of the bile to the duodenum has been impervious for some time; and then the patient is attacked with violent spasmodic pains, referable to the insertion of the ductus communis choledochus into the duodenum, and the fæces alvinæ are clay coloured.—The yellowness of the skin is not always attendant on this obstruction; and sometimes it is only partial; appearing in irregular patches on the arms, and disappearing again in the course of a few days.

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[*] "The back suffered very much in this disorder; the stoutest men complained, and even groaned under it."—Rush.

[†] I am well aware of the natural difference in the state of the pulse, as accurately described by Dr. Falconer.

"Persons, it appears, attacked by this form of the disorder, suffer more severely than in any other; and although the obstinately constipated state of the bowels renders it necessary to give very strong purgatives, yet they often require to be combined with æther and opium; nor is any relief obtained, until a copious evacuation of dark bile from the intestines take place.

"The tongue exhibits different appearances; but in general, as already observed, it is covered with a white yellowish mucus; it is very seldom dry, or brown; when the fever goes off, it often assumes a smooth shining appearance; sometimes it appears as clean as in health.

"Hæmorrhage from the lungs, stomach, and intestines, has frequently occurred, but more commonly from the latter, and very often in large quantities; it has seldom, however, been attended with any serious consequences, except in a few cases in the pneumonic form of the disorder. In several cases there has been a considerable determination to the uterus, producing frequent occurrence of menorrhagia; and that even in women, with whom the catamenia had ceased for a number of years.

"Suppression of urine is another very common symptom in this complaint, which is often attended with considerable pain, tention above the os pubis, and heat in the urethra; sometimes it happens that there is a deficient quantity, without much pain.

"The colour of the urine does not in general exhibit any particular appearance; it it sometimes of a bilious tinge, at others of a brandy colour, and not unfrequently of a coffee colour, with a copious sediment resembling the grounds of coffee; in general, however, it is not higher coloured than naturally. The appearance of the sediment is very uncertain; I have seen it take place on the fifth day, and not appear again during the remaining course of the fever.

"As before observed, there is commonly a preternatural secretion and excretion of bile; great quantities being discharged from the stomach and bowels, of different colours; green, yellow, and sometimes as dark as pitch. The vomiting which so often attends this complaint, for the most part, takes place at its commencement; although I have known it not to occur till a fortnight after the attack*. Except in the choleric form of the disease, the bowels are very torpid. This torpidity does not, I believe, arise from a deficiency of the usual stimulus of the bile; but from a vitiated state of it, producing great indirect debility of the intestines. I have known a patient labouring under this complaint, not have any evacuation from the bowels for six weeks, but what was occasioned by a purgative, which brought away at each exhibition, a considerable quantity of black bile."

To the above extract we have little to say. It is plain that under the name of synochus biliosa, Mr. White has comprehended a number of affections, which however they may resemble each other in the state of liver by which they are attended, differ in the external assemblage of symptoms, in a sufficient degree to make it questionable, whether they can fairly be comprehended under the same denomination. The different ailments which have here been described by Mr. White, may all be attended or produced by an infarction of the liver with vitiated bile, which in one case produces rheumatism, and in another hypochondriasis; but it would be just as unfair to call them both by the same name as it would be to give the same denomination to eclampsia and apoplexy, because they both arise from a morbid turgescence of the blood-vessels of the brain; or to comprehend under the same title all the various affections which arise from constipation of the bowels. This however is a dispute about words, and therefore one of trifling importance. The stream of observation is turned towards the affections of the hepatic system, and this increase of attention to the diseases of this organ will doubtless be followed by an increase of knowledge. Bile is the material with which nature dyes the above discharge, and that it is frequently altered by disease either in quantity or quality, is obvious from the fact that the fæces scarcely ever retain their natural colour in the general affections of the system. One of the most important and arduous tasks which medicine has to execute, is to connect external appearances with internal changes, and thus to enable us to ascertain those things which we cannot see by observing those things which we can see. Observation has still to do much in extending this part of our knowledge to the different states of the liver, and we lament to say, that it remains as imperfect since, as it was before the publication of Mr. White's book.

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* When this symptom appears so late in the disease, what is discharged from the stomach is of a dark colour, and forms an unfavourable prognostic.

The chief remedies which are recommended by our author are calomel purgatives, to evacuate the unnatural or superabundant bile.

In the chapter on cholera morbus we saw nothing remarkable, except that our author was not aware that after the use of diluents, the most rapidly effectual mode of exhibiting opium is in the form of clyster. This medicine if given by the mouth will frequently be rejected by vomiting, but if given by clyster will in the course of a quarter or half an hour almost invariably and completely remove all the urgent symptoms, and transfer the patient from a state of torture to a state of ease.

The remaining chapters on torpor of the liver, schirrus of the liver, and biliary calculi, need not detain us. The book concludes with a monthly list of diseases.

We turned over no leaf with so much pleasure as the last.

We have laid open to our readers the process and the ingredients by which the book has been made, and we leave them to judge of its merits. For our own parts we are unable to guess the motives which have led Mr. White to publish; not lucre, for few will buy; not fame, for few will read, and still fewer praise. We have turned over pages and waded through chapters, and met with little more than long extracts from authors which every one has in his library. We were going to advise Mr. White, but it would be all in vain. If *he* does not see the folly of writing such books *we* cannot teach it him.

ART. VI. *On Hydrophobia, its Prevention and Cure, with a Description of the different Stages of Canine Madness; illustrated with Cases.* By Benjamin Moseley, M. D. 8vo. 2s. London; Longman and Co. Cadell and Davies, 1808.

ADVERTISEMENTS, whether issuing from Gilead House, or the "house in Albany, Piccadilly," so long as they are confined to the ephemeral pages of a common newspaper, fall obviously far below the design of our critical undertaking. Even when these fugitive manifestations of the miraculous powers of their promulgators, are collected into the form of a pamphlet, to go forth the instructive and enlightening companions of the nostrums which they celebrate, we must permit them to circulate, without interposing the shield of criticism for the protection of the public against their baneful influence; but when they aspire to a classical and scientific character, and boldly advance claims to originality, where error and misrepresentation are their only distinctions, examination becomes an imperious duty.

It has not often been our misfortune to encounter so much reprehensible matter in one small tract, as we have found in the pamphlet of Dr. Moseley now before us. We feel ourselves entirely unable to decide whether presumption, ignorance, arrogance, or shameless want of candour, is the most glaring feature in this contemptible production; they constitute a whole however, unparalleled we believe in medical literature, excepting perhaps by some recent emanations from the same *dignified and candid* mind, intended to injure and degrade a practice fraught with consequences the most beneficent to the human race. So much is announced in these advertisements, or essays as the author most complacently styles them, that had we not known how little ought to be expected from such a quarter, we might have been disappointed by their inanity: still, empty as they are, it appears that many persons have been anxious to peruse them,—we are told that *Solomon's Guide to Health* has reached a *fiftieth* edition,—and therefore we may understand why "the demand for that popular daily newspaper" (The Morning Herald,) "containing these essays, was great; and the numbers printed though great, were inadequate to the demand;" and also why "many hundreds of people, who wished to obtain an impression of one or more of them, were disappointed." How trivial this disappointment compared with that which must have followed a successful application! We are next favoured with the following sententious and instructive jumble of motives, falsehoods, condemnations, reasons, and the like, which we transcribe as a specimen of the luminous and aphoristic, as well as for the purpose of applying to some parts of it a little gentle chastisement.

"On this account," says Dr. M. "I have been solicited to reprint these detached essays, and to publish them *all* together.

"This must be my apology for the method in which they make their appearance; but the exigency of circumstances may also plead in my favour. *Acts devoted to public good* in times of danger, rise above all forms; and are paramount to all personal considerations.

"The two cases will in many minds occasion the deepest reflection.

"It may seem incredible to some people, that there has *never* been an accurate description of hydrophobia, and a succession of the symptoms of canine madness *before given to the world.*

"It does not appear satisfactorily to the learned," (to the *learned author* we suppose,) "that *any writer* on this malady *ever* saw its whole train of evils. The ancients, like the moderns, describe and prescribe in their own way; but not one among them even pretends to have seen hydrophobia with rabies in all its stages.

"Some of the moderns assert they have witnessed the dreadful scene; but their own evidence is against them. BOERHAAVE is *bad;* MEAD is *worse;* and the rest with very few exceptions are *contemptible.*

"It has been a theme for theoretic physicians, a metaphor for poets, and a fable for historians.

"There can be no doubt that many people have seen the disease; *because* many have perished by it. But it may be asked what are these spectators' descriptions, and their details of practice?"

We do indeed admit that it must seem *incredible* to some, and by no practitioner acquainted with the subject we venture to assert, can it be *believed*, that until the descriptions of Dr. M. appeared, the disease had not been described. We cannot suppose that any thing *lately* published should be known to one whose learned propensities have confined him exclusively to the stores of ancient wisdom; who has been engaged in scrutinizing the pages of Appolloneus Pergamenus and Scribonius Largus, of Oribasius and Ægineta; who has been able by profound research to decide that Hippocrates has not written one word about hydrophobia, and that Homer amongst all the abusive epithets applied to Hector, his half-favoured but furious hero, has never once called him a *mad dog*. When Dr. Moseley can prevail upon himself to descend from these erudite occupations, and more especially when he shall have completed those " acts devoted to public good," in which he has been for a few years so assiduously, so usefully, so benevolently engaged,—acts which a threatened review of the report of the college of physicians on vaccination shews to be happily yet in progression,—we hope that he will allow us to direct him to a few sources of information, which in our more humble and limited studies we have discovered. The Narrative of Mr. Bellamy's sufferings given by Dr. Fothergill, we can recommend as one of the most unaffected, impressive, and complete delineations that has been furnished by any writer: the Three Cases published by Dr. Vaughan will also be found very comprehensive and instructive: the minute and picturesque Histories by Dr. Babington, [Medical Communications, Vol. II. and Medical Records and Researches,] must be consulted with profit and advantage even by Dr. M; as well as the Cases so ably detailed by Drs. Ferriar and Bardsley. Unwilling as we are to burthen our erudite author with too many references to the "contemptible" writings of the moderns, we cannot avoid recommending to his notice the second part of the Memoirs of the Royal Society of Medicine of Paris for the year 1783, consisting chiefly of treatises on the subject of rabics canina, and some of them truly important; and *instar omnium*, the elaborate work on hydrophobia, by Dr. Robert Hamilton, containing a summary of all the valuable information which had appeared prior to the superlative little tract now under review.

The examination which we have suggested must prove satisfactorily even to Dr. M. that not one essential circumstance has been noticed by him which former " spectators" had not seen and detailed; and consequently, that his "*new, plain,* and *correct* statement of the different stages of the disease" will afford no further occasion for self-gratulation. Indeed it is somewhat extraordinary for one who boasts of having *seen* so much, that in each of the two cases "the symptoms arising in the system antecedent to hydrophobia of that description called nervous," (vide last page,) are detailed from the accounts of the patients themselves, both being hydrophobous when the Dr. first saw them; and that in one, the case "successfully treated," the two last stages, *dyscatoposis* and *rabies* did not occur, *because* of the measures most judiciously employed for their prevention. These imperfections in observation however, the fine imagination of the author has we doubt not abundantly supplied. In regard to the stages of this disease we may remark, that although as we have just mentioned, the earliest symptoms are stated to be nervous, and therefore the *nervous* ought certainly to be the first of the stages, yet is the disease divided into the three following: "first, *hydrophobia*, or the dread of water; second, *dyscataposis*, or difficulty in swallowing and choking; third, *rabies*, or convulsion attended with vehement spitting and frothing at the mouth:" in what manner this arrangement may have been derived from the second case we have just hinted, and how well it corresponds with the first will appear, when it is known that FREDERICK MICHAEL METCALF, the patient, informed Dr. M. that "he was attacked about four o'clock the preceding morning with *a difficulty in swallowing any liquid*, which he first perceived when he attempted to drink some porter, the remains of half a pint which he had on Saturday evening." But trivial inconsistencies must be overlooked in the towering research of great and original minds.

In addition to the new statement of the stages of this formidable disease, we are likewise favoured with a new view of the causes which have prevented the attainment of a knowledge of its cure. Whatever we may be inclined to think of the alledged originality of other parts of the pamphlet, in the valuable and brilliant opinion which we subjoin we firmly believe the author has not been anticipated.

"Another misfortune to humanity is, that since the separation of physic and surgery into distinct professions, every physician and every surgeon *are* not qualified to undertake complicated cases—such as wounds from rabid and venomous animals with their consequent diseases."

"A surgeon now is supposed to know as little of physic as possible; and a physician is supposed to understand about as much of surgery. What then must be the fate of a patient in the hands of either, who requires the utmost aid of both."

"*Hence it is* that the knowledge of hydrophobia, and the treatment of the bites of mad dogs in particular, have not advanced with other improvements in medicine. *They* are as ill understood as *they* were in the time of CELSUS; nay, worse; *because* CELSUS *was as renowned in surgery as in physic:* and his method was not without great design in both."

Our readers must be desirous to know what have been the fruits of this "renowned" combination, the want of which has occasioned to humanity misfortunes so much to be deplored. The important information we cannot convey more intelligibly than in the words of Dr. Moseley.

"CELSUS considers hydrophobia as an hopeless misery. *The only remedy,* he says, is plunging the patient unawares into a cold bath, and there half drowning him. I believe CELSUS, who first mentions this process, never tried it. He knew and mentions the dangers of it. Yet this has given rise to all the erroneous notions concerning cold bathing as a *preventive* of hydrophobia."

Celsus has, it must be admitted, spoken also of the use of wine, and has greatly commended the employment of oil, which we suppose may rank with the burnt river crabs extolled by Galen, or the pepper, aromatics and boles applauded by others.

That the author is a surgeon-doctor we are no where directly told, but by easy inference we are conducted to the knowledge that it is the case. The want of this "renowned" combination being the cause of the lamentable want of success in the treatment of this dreadful malady, (as has already been *fully proved,)* and Celsus, in whom it so distinguishingly existed, though approaching the nearest to perfection having notwithstanding, failed of producing a cure, it follows *a fortiori,* that in Dr. M. who has actually succeeded, the ingredients must be found combined in the best and happiest proportions. What, it will be eagerly inquired, were the novel and profound instructions issued on the occasion by this "renowned" chirurgo-medical practitioner? These the doctor shall himself proclaim.

"I desired her *to go* immediately *home,* and *to go to bed.*"

"I should have had doubts of the utility of applying *my* usual caustics, *butter of antimony* and *lapis infernalis* (the *cure* in recent cases) to the bitten parts, which were healed in three days after the accident, and had now no other visible remains than two small indentations of the skin—but that the state of the finger and hand shewed the virus was still active in the parts adjacent to the original wounds. Therefore I removed the skin where the wounds had been and instituted a drain, which was continued through the whole process of treatment."

"At five o'clock in the afternoon I had an ounce of the *unguentum mercuriale fortius* well rubbed in by herself, about her neck, throat, thighs, and legs. I then gave her the following draught with directions to promote perspiration every way possible.

℞ Julep. e Camphora ℥ ij.
Spt. Volat. Aromat. ʒ j.
Rad. Valerian. recent. pulv. ʒss.

This draught was repeated every four hours."

In the course of the night considerable perspiration had occurred, and the next day another ounce of the ointment having been used early in the morning, the breath became offensive, and the gums sore. In the space of forty-eight hours four ounces of ointment were employed, and the salivation induced was interrupted by powerful action on the bowels occurring on the third day; this effect being obviated, it became again copious, and continued to be so for twelve days. The patient "is now (about seven weeks from the commencement of hydrophobia) in perfect health," says Dr. M. "and I am under no apprehension concerning her safety."

Now what mere physician, we triumphantly inquire, could in these chaste times have ever thought of mercury in any form? and especially have had the slightest conception of the mode of applying mercurial ointment? Or what man, merely a surgeon, could have had the most distant idea of administering camphor julep and sal-volatile? And as to *valerian!* 'tis too absurd for a moment to suppose, that he should have the slightest notion of valerian! Nay we are of opinion, that should the patient have enjoyed the trivial advantage of the simultaneous assistance of the most able physician and the most able surgeon, they would be so far from equivalent to one surgeon-doctor, that it would be next to an impossibility for them both, to advance to the sublime comprehension of *lapis infernalis and butter of antimony!!* Such are the transcendant, the incalculable advantages of consulting that most splendid and "renowned" of all medical characters,—a surgeon-doctor.

It cannot be imagined, that Dr. M. would take the trouble to look into the writings of the moderns which he instinctively knew to be contemptible, and therefore he cannot have learned that Asti, an Italian physician, supported a salivation by the free use of mercury, at an early period of the disease for twelve days, and that the termination was fatal; or that mercurial inunction had been most actively employed in France as a prophylactic, the introduction of the mercury having been accelerated by the use of the warm bath, yet with no better success; nor can he be supposed to have met with the many accounts of its unavailing employment which this country affords. Doubtless he has never heard that camphor has been often tried in much larger doses than two grains, (the quantity contained in two ounces of the mixture, prepared according to the pharmacopœia) nor that ammonia in various forms and proportions has been frequently used in vain. He could not know that the *potio antilyssa* of Selle, consisting of theriaca, sal cornucervi, camphor and cantharides, from which the best effects in one case of hydrophobia are said to have been experienced, must have been deemed equal to his draughts, if for the less active ingredient cantharides, valerian had been substituted. These things he cannot have known, otherwise his candour so conspicuous on some former occasions, would not have allowed him to be silent. We do not intend to insinuate that Dr. M. is culpably ignorant of the subject on which he has written: such an insinuation we must be far from making, for we firmly believe that his great candour, which we have already confessed, can alone be equalled by his knowledge.

We are informed, that "this is the only case of complete hydrophobia, from the bite of a mad dog, successfully treated, on record." We beg leave to hint to Dr. M. that a case was published by Dr. Nugent of Bath, in the year 1753, wherein "the dyscataposis or difficulty in swallowing, and the choking had begun their dreadful parts of the tragedy," which was "successfully treated" by repeated vensection, and the liberal use of the Tonquin powder, consisting of musk, with cinnabar native and factitious. But the success of Dr. M. it appears is not confined to a single case, for speaking in reference to the two cases published, he says, "these are not the only patients I have had under my care, for the bites of mad dogs, within the last few months. I wish they were. It is well known I have had *many*; and have *cured* them. That is, the hydrophobia has been *prevented*." Not to insist on the novelty of this attempt to identify prevention and cure, we may observe that we are not at all surprised by the declaration that many have been cured of hydrophobia. We recollect to have seen Dr. Moseley's name conspicuously placed on large bottles containing Jamaica ginger, "prepared according to his direction," and on packets containing Rhubarb pills, "prepared according to his prescription;" he may consequently be ranked amongst those public benefactors of mankind who cure all *incurable* disorders.

There is a species of *rabies* most dreadful in its consequences, and we fear in its nature incurable, to which we are solicitous that mercury and *valerian*, with the remedy of Celsus (a ducking) should be most vigorously applied. This species may be denominated rabies *antivaccina*. It has only been observed during a short period, but has already acquired a prodigious degree of malignity; and its effects as testified by the bills of mortality alone have been so extensively destructive, that if not exterminated, it will in a few years occasion deaths far more numerous than can ever be compensated for by all the anti-hydrophobic energies of mercury and valerian. Of this kind of madness we have a few savage examples; and these in order to put the remedies to the most severe test, ought to be selected as the subjects of experiment. We therefore recommend to *Moseley* and *Birch*, to *Squirrel* and *Adams*, who are all most severely afflicted, and all equally anxious to promote general in preference to individual good, immediately to take advantage of the light now afforded: and that the plan may be pursued with activity and perseverance proportionate to the extent of the calamity, we farther advise, that they should frequently meet. Acting thus in concert they would stimulate each other; and such meetings from their perfect congeniality and the similarity of their pursuits, would no doubt be highly gratifying and interesting. *Asinus asino, sus sui pulcher.*

ART. VII. *On the Structure and Uses of the Spleen.* By Everard Home, Esq. F. R. S. Philosophical Transactions for 1808, Part 1st. 4to. London; Bulmer and Co. 1808.

HOW great was the instruction anticipated from the perusal of this paper, when on opening the volume we found that the Copleyan medal had been adjudged to Mr. Home, can only be understood by those who know, with what exquisite discernment of superior merit this honour is annually bestowed, and what trivial claims have appeared in his former communications to this learned body. Perhaps we may be inclined to think the investigation of the accommodating powers of the eye superior to any since presented; but as the adjudicants, the President and Council, in common with many others are well aware, that whatever mechanical ingenuity or optical science might be displayed, was ascribable to the late Mr. Ramsden alone, the reward could not then be merited. We are not certain however that the discoveries respecting the stomach, lately communicated to the Royal Society, may not have made a very strong impression on the scientific and penetrating mind of the President, as announcing a division of that organ without any dividing cause, and a contrivance without any conceivable purpose to be answered by its institution.

As it had been discovered by Mr. H. "during the investigation of the functions of the stomach, that while digestion is going on there is a separation between the cardiac and pyloric portions, either by means of a permanent or muscular contraction,"* it became requisite to make another discovery: the discovery of a route through which fluids, poured into the stomach in quantities too ample for the convenience of digestion conducted according to the new plan, might make their escape. This office of waste-pipe to the stomachs of the over-thirsty, was with much hypothetical ingenuity assigned to the spleen; an organ which beyond every other has been devoted to the tortures of speculative absurdity.

"This fact," (the separation before mentioned) "placed the process of digestion in a new light, and led me to consider in what way the quantities of different liquors, which are so often taken into the stomach, can be prevented from being mixed with the half-digested food, and interfering with the formation of the chyle.

"Pursuing this inquiry, I found that the fluids are pincipally contained in the cardiac portion, and the food that has reached the pyloric portion is usually of one uniform consistence, so that the fluids beyond what are necessary for digestion would appear to be carried out of the stomach, without ever reaching so far as the pylorus. To ascertain the truth of this opinion is the object of the present paper.

"The lymphatic vessels of the stomach are numerous, but they are equally or more so in the other viscera. Many circumstances appeared to render it probable that the *spleen is the route by which liquids are conveyed.* The more I considered the subject, new reasons in favour of this opinion crowded on my mind, so as almost to enforce conviction, and made me set about *devising* various methods by which its truth or falsehood might be established."

Respecting this singular and improbable division we wish to obtain some proofs, in addition to those which Mr. H. has advanced, before we can admit its existence; much less allow it to become the basis of other opinions equally singular and improbable. Bodies have often been examined as early as by Mr. H. without this appearance having been observed; and certainly the stomach has often been inverted without exhibiting such a strong provision of muscular fibres towards the smaller extremity. We cannot help fearing that the anatomical assistant Mr. Brodie, of whose skill Mr. H. has very advantageously availed himself, and the artist Mr. Clift, whose abilities as an anatomical draughtsman are unrivalled in this metropolis, have both been apprized of the wishes of their employer, and have dissected and delineated accordingly. Besides, admitting the muscular fibres of this portion of the stomach to be arranged as in the figure, we cannot obtain a pyloric cavity as it may be termed, without gratuitously admitting a degree of contraction in the ring nearest the cardiac extremity, much greater in proportion to the contraction of the rings adjacent than we are at liberty to believe; and should all the fibres act equally, the part will form a path for the digested matter moving towards the duodenum, rather than a sac in which it may be detained to undergo a separate process. The performance likewise of two distinct and regular processes in spaces, produced by the casual contraction, and division of a viscus so much like a single cavity both in figure and design, as the human stomach, we cannot easily comprehend: it is true that in some animals there is an appearance of the organ consisting always of two parts, but even the offices of these parts may be the same, and should they be different, it does by no means follow that they must be different in the stomach in which there is no such structure. When Mr. H. has accidentally met with the hour-glass contraction of the uterus, he has not we presume, concluded that it was universal and necessary to the expulsion of the fœtus. The arrangement of the solid contents of the stomach at the pyloric extremity, and of the fluid at the cardiac, seems somewhat difficult to understand, without adopting an inversion of the laws which regulate the distribution of fluid matter, or without the assistance of a peculiar cribriform process *hereafter* to be discovered, permitting the passage of solids and resisting that of fluids. Other objections to the adoption of this whimsical opinion present themselves, but as it is not the leading object of these remarks, which are introduced merely in consequence of the reference contained in the quotation before inserted, we shall for the present dismiss it.

In endeavouring to establish or refute the hypothesis before stated, the first point to be decided, says Mr. Home, was, whether "the liquids received into the stomach do escape in any considerable quantity, when prevented from passing out into the pylorus;" this by securing the outlet was easily ascertained. Mr. H. accordingly closed the pylorus of a dog, and injected into the stomach five ounces of water, coloured by indigo, all of which was afterwards found adhering to its internal coat. In half an hour the dog vomited to the extent of two ounces, and was then destroyed. The pyloric portion of the stomach was found empty and contracted, but the cardiac contained two ounces of solid matter with one ounce of water; and thus if we allow for the quantity of fluid which "it is reasonable to suppose there was in the solid food," the quantity which had escaped must have exceeded two ounces. Similar experiments with similar results were performed with a decoction of madder and an infusion of rhubarb, Mr. H. knew, it seems, that the former of these substances imparted a bright colour to the bones of animals, but here his knowledge ended, otherwise he could not have felt disappointment on finding that the colouring matter, when simply united to water in the decoction, gave a different tint from that of the same matter precipitated in combination with phosphate of lime. His own chemical information not having befriended him in the suggestion of indigo and madder, Mr. H. very properly resorted to Mr. Brande thus adding to his anatomical and graphical assistants a chemical auxiliary; and rhubarb as being detected in small quantities by an alkali, was judiciously chosen. Many trifling experiments are next detailed, made by adding a few drops of tincture of rhubarb to water, serum, and urine, together with the observation of the time required for its appearance in the urine, when taken betwixt meals, and when taken immediately before a breakfast of tea; also of the time required to give a *decided* tinge to the fœces. An experiment was then performed in the manner already described, with diluted tincture of rhubarb, and nine hours afterwards the dose being occasionally repeated, the dog was killed; part of the fluid had escaped, and the spleen was turgid as in the former instances; the cells which the author appears not to have known previously, and which he almost ventures to think *he* has discovered, were very large and distinct; the urinary bladder was full, and the urine subjected to the test, exhibited a deep red colour. A piece of the spleen macerated in water imparted so much rhubarb as to be discoverable by the test, whilst a piece of the liver treated in the same manner imparted nothing; and in this as in the former experiments it is observed, to shew we suppose, that absorption had no share in the business, that "none of the absorbent vessels passing from the great curvature of the stomach were in a distended state so as to be rendered visible."

"The discovery of this fact," the direct communication betwixt the stomach and the spleen, is thus accomplished, and such are the proofs on which at first Mr. Home consented to let it rest. It surprises us not a little, that Mr. H. should say, "as there was no appearance of rhubarb in the liver, it could not have arrived there through the medium of the common absorbents carrying it into the thoracic duct, and afterwards into the circulation of the blood," because he had proofs of its existence in the circulating mass, by the change produced in the urine. Perhaps, indeed, another discovery, of a direct connexion betwixt the spleen and the bladder, may be made to accommodate this stricture, which rendered the fisrt necessary; for in the hands of Mr. Home, every *stricture* must become exceedingly productive, either of theory or something more substantial. We are informed in a subsequent essay, that rhubarb appeared in blood taken from the splenic vein, when none could be detected by macerating the liver; who but Mr. H. could have failed to see in dilution or the action of the gland on the colouring substance, the cause of its disappearance? It surely will not be contended, that because the absorbents were not rendered visible by distention, that no additional quantity of fluid could have passed through them, because as no other connecting tubes are alledged to have been seen, the objection is equally strong, against any that may be imagined for supporting the hypothesis.

From the very unsatisfactory quality of these experiments, we were prepared to expect the "further experiments on the spleen," to be found in a second paper, read about two months after the former. It was the intention in this series, to determine whether the same communication takes place when the subjects are in a natural state, and the ass was chosen as the fittest animal for the purpose. Another auxiliary Mr. Sewell, was also added, of whose "personal aid" Mr. H. as usual speaks with high encomiums. The assistants who we suggest with deference, must when united be nearly equal to the principal, are arranged as follows;—Mr. Sewell, Mr. Brodie, Mr. William Brande, and Mr. Clift. They may also be considered a competent number of witnesses for establishing the truth of the narrative.

These *asinine* experiments six in number, were made with as many animals, some of which were kept without food twenty-four hours; some two days; and one without any fluid for four days: they were then liberally drenched with tincture or infusion of rhubarb, and in due time mercifully *pithed* by Mr. Sewell, we suppose, for the purpose of examination. The same report of the spleen itself is given as before, and the following extract will convey sufficient information respecting the result of the experiments:

"Infusions were made of the spleen and liver, under similar circumstances; they were strained off into separate glasses, and tested by the alkali. The urine was tested in the same way. The serum from the different portions of blood, was also poured off into separate glass vessels, to which the test was added. In nineteen hours after the blood had been taken from the veins, they were all compared together. The urine had so deep a tinge, that it nearly resembled the pure tincture of rhubarb in appearance: the others had a tinge, although in very different degrees: the quantity of rhubarb they contained, was estimated by adding tincture of rhubarb to alkaline water, so as to produce corresponding tints. The infusion of spleen had a tint equal to sixty drops of tincture of rhubarb in two ounces of alkaline water; the serum of the splenic vein to fifteen drops; the serum from the left auricle of the heart, to three drops. The infusion of the liver gave no orange tinge: but had it not been obscured by the red particles of the blood, it must have been equal to that of the serum from the auricle."

All these tints seem to us to give no colour to any other supposition, than that the kidneys separate a great deal of rhubarb from blood, in which it is dissolved; that the spleen separates some, although not so much; and that the liver may or may not, separate a little. If the intensity of colour in any individual cell of the spleen, establishes the direct connexion of that cell with the stomach, how much more close must the connexion of that large cell the urinary bladder be, in which a fluid almost equal in colour to tincture of rhubarb itself, was found. We cannot understand why Mr. H. if he wished to investigate the subject thoroughly, did not macerate the kidney, and tint the blood in the emulgent artery and vein: the colour of the blood in the splenic artery and vein, ought also to have been contrasted; and many other things ought to be done, before this insignificant hypothesis can be allowed to be fairly "tested" by experiment. Far be it from us however, to attempt to provoke Mr. H. or any of his assistants, to sacrifice one dog, or one ass more, to a purpose so vague and unproductive.

The spleen as many of our readers well know, has long been felt to be the *opprobium anatomiæ et physiologiæ*; and from this disgrace, we confidently predict, that it will never be rescued by the genius of Mr. Home. It has in various modes, been esteemed an organ subservient to the stomach, such as acting the part of a "lamp placed under the stomach, to warm it by its sparkles, like a culinary fire placed under a pot;" and at length it obtains the dignified office of a sewer, through which the waste or refuse water of the stomach makes its escape. This idea we acknowledge, is rather less absurd and libellous, than that which places it on the left side, merely to counterbalance the weight of the liver on the right. Occasionally in former days, the spleen was more honoured; for Van Helmont scrupled not to call it *Sedes archei, veneris nidus, hospitium animæ;* whence the expression, *lienosi pro ingeniosis habentur*. But we do not propose at once, to recite all the dreams which we have collected, as we expect that our attention will ere long be recalled to the subject, this viscus being doomed to become the seat of the most genuine visions of the most incorrigible speculators. "Another and another still succeeds, and the last fool is welcome as the former."

MEDICAL AND SURGICAL INTELLIGENCE,

Consisting of brief Communications of Varieties in Disease, and Novelties in Practice is requested to be addressed to the Editors, at Messrs. Longman and Co.'s, Paternoster Row.

Cases of Hydrophobia.

Art. 1. A case of hydrophobia has lately been printed for private circulation, by Dr. Powell. It conveys a very minute and distinct account of the symptoms, which did not differ essentially from those generally observed, narrated in a very plain and most instructive manner; it forms an important addition to the histories of this disease already published. *Argentum nitratum* was the medicine employed, because used with advantage in some other diseases of irritability and irregular convulsions. The termination was fatal: a minute dissection was not permitted: nothing unusual occurred in the examination which was instituted.

A highly interesting and picturesque description of the same disease, lately attended by Dr. Marcet of Guy's Hospital, will soon appear in the Transactions of the Medical and Chirurgical Society of London. This case terminated like the former. The mineral solution, *(Fowler's solution of arsenic,)* was very largely exhibited.

Two fatal cases of hydrophobia have lately been admitted into the London Hospital; and another has fallen very recently under the care of Mr. Carlisle, in which some singular circumstances have been noticed; besides several more of doubtful authenticity mentioned in the public prints, of which two were said to be occasioned by the bite of the same dog, which produced the disease in one of the patients at the London Hospital.

Remarkable Case of Cow pox.

Gentlemen,

Finding you have allotted a part of the London Medical Review for communications of varieties in disease, I have forwarded you a curious case related simply as it occurred in my practice, without offering any comments. Should it be thought of sufficient importance for insertion, you are at liberty to relate it in any form you may think most convenient.

I am, Gentlemen,
Your obedient, humble Servant,
JOHN MITCHELL.

Kington, Herefordshire,
3 Oct. 1808.

Art 2. A lad fourteen years of age was inoculated with the vaccine disease in his left arm, which went through its course in a very decided and regular manner; on the ninth day he had a slight attack of fever, the axillary glands enlarged and painful, continuing about two days; the disease receded from that time, and on the twenty-first day the scab had fallen off. Seven days after this time, and twenty-eight from the day of inoculation, the lad had a smart attack of fever, more violent than that which occurred on the ninth day. Upon investigating the cause, it evidently arose from three vaccine vesicles, of the size of those of the ninth day by inoculation, formed upon the lower part of his loins. The absorbent glands in each groin were swelled and painful.

N. B. No matter could possibly have been applied from any other vesicle to the skin upon his back.

Cases of Aneurism.

Art. 3. Two cases of aneurism remarkable for their similarity have occurred within the last month in London. In both instances more than one sac was found in the course of the aorta, and in both the ruptured sac was formed before this artery quitted the pericardium. This capsule was by consequence perfectly distended with blood. Both patients had popliteal aneurisms; the death of one took place after the operation for this disease had actually commenced; and of the other only a short time prior to its intended performance. The age of the former was 40 years, that of the latter 60.

During the month of August last, an instance of equally sudden death occurred in St. Bartholomew's Hospital two days after the patient's admission, who had an aneurism of the ham, and was aged 37 years.

There is at present in anotherof the hospitals of the metropolis, ayoung, and to all appearance, healthy man, who has an aneurism in each groin. That in the left has a very extensive sac, and upon applying the finger to that part of it opposite the artery, the jet of the influent blood is distinctly perceived. That in the right is small and circumscribed.

A carotid aneurism in a young woman will shortly become the subject of operation.

We have lately seen a specimen of the aneurismal structure, interesting inasmuch as it establishes the now-a-days disputed fact of an occasional dilatation of the artery.—The enlargement commences abruptly by a line of osseous matter; the sac, which would contain the fist of a midde-sized man, is lined throughout with flakes of bone, and though the internal coat of the vessel which is thus patched is extremely thin and brittle, it does not on minute inspection any where exhibit a solution of continuity. It is worthy of remark, that all the valves, coronary arteries, &c. are perfect and natural.

Lusus Naturæ.

Art. 4. Another extraordinary lusus has been very recently met with by some practitioners of eminence, in the body of a dropsical female. It consists in a much enlarged ovarian cyst, containing balls of a matter somewhat resembling bees wax, and considerable quantities of hair. The particulars will of course be published.

Our readers will in a few weeks be gratified with the perusal of the first volume of the Records of the Medical and Chirurgical Society of London. It will contain some very valuable contributions to medical and surgical science, from practitioners of first-rate eminence in the metropolis.

We have received Dr. Cheyne's letter, and the present number was intended to have conveyed our answer in the shape of a review of his Essay on Hydrocephalus Internus. We regret that an unforeseen accident has obliged us to postpone it to our next, which will contain in addition, criticisms of Mr. Davy's Bakerian Lectures; Messrs. Aikin's Chemical Dictionary: Dr. Jackson on Affusion in Fevers; Mr. Allan on Lithotomy; Mr. Watt on Diabetes, &c. &c.

LIST OF MEDICAL PUBLICATIONS.

Remarks on the present State of the Lunatic Asylums in Ireland, and on the Number and Condition of the Insane Paupers in that Kingdom; with an Appendix, containing a Number of Original Letters and other Papers connected with the Subject. By Andrew Halliday, M.D. 8vo. 3s. 6d.

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Page 300, line 25, *for* lacerated, *read* luxated.

301, line 38, *for* in consequence of a lever, *read* in consequence of such a lever,

---

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